THE

GEOLOGICAL MAGAZINE.

NEW SERIES. DECADE III. VOL. IX.

No. X.—OCTOBER, 1892.

ORIGINAL ARTICLES.

I.—Further Additions to Australian Fossil Echinoidea.

By J. W. Gregory, B.Sc., F.G.S.;
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(PLATE XII.)

DURING the last two years four papers have appeared upon the Australian Cainozoic Echinoids, contributed by M. Cotteau,[1] Prof. R. Tate,[2] Herr A. Bittner,[3] and myself.[4] These add considerably to our knowledge of the fauna; they show that it is Eocene and Oligocene, instead of Miocene, and that it is remarkably varied and rich in genera. I am now able to add one or two more species to the list, and at the same time take the opportunity of referring to one or two changes proposed by Prof. Tate and Herr Bittner.

Fam. Laganidæ.

Genus, *Laganum*, Gray, 1825.

J. E. Gray. "An attempt to divide the Echinida. . . . ." Ann. Phil. 1825, vol. x. p. 428.

Species, *Laganum decagonale*, Lesson. Var. *rictum*,[5] n. var. Pl. XII. Fig. 1.

Synonymy (of the species). See A. Agassiz. Revision of Echinoids. Illus. Cat. Mus. Comp. Zool. No. 7, 1872, p. 148.

Scutella decagonalis, Lesson, 1827, in Poiret. Scutello Dict. Sci. Nat. t. xlviii. p. 229.

Lagana decagona, Lesson, 1834, in Blainville Actinologie, p. 215, pl. xviii. fig. 3.

Laganum decagonum, Lesson. L. Agassiz. Mon. Echinod. ii. Scutelles, 1841, p. 112, pl. xxiii. figs. 16–20.

Diagnosis (of the variety).

Form: elongated elliptic; the posterior end is longer and narrower than the anterior; the anterior end is semicircular; the sides taper backward. The base is flat; the margins are tumid, and are separated from the slightly conical apex by either a flat platform or a slight broad depression.

[1] G. Cotteau. Echinides nouvelles ou peu connues. No. IX. Mém. Soc. Zool. France, t. iii. 1890, pp. 546–550, pl. xii. figs. 13–18. No. X. *Ibid.* t. iv. 1891, pp. 629–630, pl. xix. figs. 10–14.

[2] R. Tate. A Bibliography and revised list of the described Echinoids of the Australian Eocene, with descriptions of some new species. Trans. Roy. Soc. South Australia, vol. xiv. pt. 2, 1891, pp. 270–282.

[3] A. Bittner. Ueber Echiniden des Tertiärs von Australien. Sitz. k. Ak. Wiss. Wien, Bd. CI. Abt. i. 1892, pp. 331–371.

[4] J. W. Gregory. Some Additions to the Australian Tertiary Echinoidea. Geol. Mag. Dec. III. Vol. VII. 1890, pp. 481–492, Pls. XIII.–XIV.

[5] From *ringor*, *rictus*, to open the mouth widely.

Ambulacra: Petals extend two-thirds of the distance from the apex to the ambitus; the lateral pairs are equal in length; the anterior ambulacrum is the longest. The petals are sharply closed below; the width of the pore area expands rather gradually to the distal end of the petal, then is there closed somewhat abruptly. The interporiferous areas are large, and taper slightly to the blunt distal end.

Apical system: at the apex of the test. The madreporite is raised, large, and central. There are four large genital pores. Of the radial (ocular) pores the right antero-lateral is very large; those of the right postero-lateral and left antero-lateral ambulacra are small; the left postero-lateral pore is not developed.

Peristome: mouth somewhat pentagonal; large; the width is half as much again as the length. It is situated before the centre. There are no interradial actinal furrows.

Periproct: the anus is large and almost circular; it is close to the margin.

DIMENSIONS.

Length			68	millimètres.
Width			55	,,
Height			12	,,
Mouth: width			6	,,
length			4	,,
distance from anterior margin			31	,,
Apical system: distance from anterior margin...			31	,,
Petals: anterior:	length		22	,,
	width		6	,,
antero-lateral:	length		20	,,
	width		6	,,
postero-lateral:	length		20	,,
	width		6	,,

DISTRIBUTION.—Cainozoic. Shark's Bay, West Australia.

Collected by HARRY PAGE WOODWARD, Esq., F.G.S.

AFFINITIES AND DIFFERENCES.—The specimen on which this species is founded is unquestionably a very close ally of *Laganum decagonale*, Less., though, as to whether it should be regarded as a variety or a distinct species, I do not care to express an opinion on a single specimen. It differs from that species by the elliptical and somewhat pentagonal shape of the mouth, and the absence of the five interradial furrows which radiate from the mouth. The British Museum contains a large series of specimens of that species, but the circular form of the mouth is constant; the actinal depressions do vary in degree of development, but I have not seen one in which it is not quite distinct. These two characters may not improbably be of specific value.

The shape of the test differs from the normal decagonal form; but some specimens of the species have a form identical with the fossil.

Herklots figured[1] a specimen from the Java Tertiaries as *Scutella decagona*, n. sp., Martin[2] referred this to *Peronella decagonalis*, Ag.,

[1] J. A. Herklots. Fossiles de Java. Pt. IV. Echinodermes. Leyden, 1854, p. 9, pl. i. figs. 6, 6*a*.

[2] K. Martin. Die Tertiärschichten auf Java. Anhang. Revision der von Herklots herausgegebenen fossilen Echiniden Java. Leyden, 1880, p. 3.

and included *L. angulosum*, Herklots,[1] as a synonym; but as Herklots neither figured nor described the actinal side, a certain amount of doubt must remain as to the accuracy of this determination. As the present variety differs from the previously known species in the same points as *L. decagonalis*, it need not be compared more closely with them.

Family, Cassidulidæ.

Genus, *Cassidulus*, Lamarck, 1801.

Système des Animaux sans Vertèbres. 1801. p. 348.

Species, *Cassidulus florescens*, n. sp. Pl. XII. Figs. 2–4.

Floresco, to begin to blossom, referring to the imperfectly developed floscelle.

Diagnosis.—Outline seen from above elongated, tapering to the anterior end; the greatest width is at the distal end of the postero-lateral petals. The margins are long and fairly straight. The ends are well rounded. Seen from the side it appears evenly rounded, except for the flattened posterior slope. The ambitus is tumid.

The actinal surface is concave; the peristome occurs at the summit of the depression. The median bare band is imperfectly developed.

Apical system: before the vertex. A large central madreporite and four genital pores.

Ambulacra: Petals sublanceolate, flush; open below. The anterior is the longest. The antero-lateral pair is considerably shorter than the postero-lateral pair.

Peristome: anterior. Floscelle not well developed; the bourrelets are massive, but not prominent. Mouth pentagonal.

Anus oval: broad and large. The subanal groove shallow, short and broad.

Dimensions.

Length	22 mm.	19·5
Width: at apical system	16 ,,	14·5
maximum	17 ,,	15·5
Height	9 ,,	8
Distance of apical system from anterior end ...	9 ,,	8
,, vertex ,, ,, ...	11 ,,	10
,, mouth ,, ,, ...	10 ,,	9

Distribution.—Fyans Ford Hill, Moorabool River; 1½ miles N.W. of Geelong. Middle Murravian. (Upper Eocene.)

Type.—Brit. Mus. E. Presented by T. W. Reader, Esq., F.G.S.

Affinities and Differences.—This species is most clearly allied to the Lutetian (Upper Eocene) *Cassidulus faba*, Defr.[2] The most important difference between them is in the structure of the petals: these are longer and almost entirely closed in the French species, whereas in the Australian form they are almost open; the anus is also longer and narrower and the test higher. Its shape resembles that of *Echinobrissus vincentinus*, Tate, but there is no reason to doubt the accuracy of Prof. Tate's generic determination.[3]

[1] J. A. Herklots, *op. cit.* p. 8, pl. ii. fig. 4.

[2] Defrance Dict. Sci. Nat. t. vii. 1817, p. 227. For figures and synonymy see Cotteau, Pal. Franç, Echinides Eocènes, t. i. 1887, pp. 510–512, pl. 139.

[3] Tate, *op. cit.* p. 280.

As the species is associated at Fyan's Ford with *Sarsella forbesi* (Woods and Dunc.) *Monostychia australis*, Laube, and a specimen which is probably a young *Cassidulus* (*Australanthus*) *longianus*, Greg., there can be no doubt of its age.

The reference to the last named species necessitates the consideration of the genus *Australanthus* recently founded by Herr Bittner for an Australian species described in 1890 as *Cassidulus longianus*. Herr Bittner regards his genus as apparently most allied to *Hardouinia*, and as intermediate between *Cassidulus* and *Breynella* (*Echinanthus*, Desor *non* Leske). In 1890 I followed Desor in not accepting *Hardouinia*; and as I regarded the new species as occupying the same position in relation to *Cassidulus* as d'Archiac and Haime's genus held to *Echinanthus*, it did not seem desirable to found a new genus for the species. The late Prof. Duncan, however, finally accepted *Hardouinia* as a sub-genus, and as it is also adopted by M. Cotteau and Herr Bittner, the current opinion is strongly in its favour. As *Breynella* is a large genus, there is much to be said on the grounds of convenience for any sub-genera that are based on characters that are at all satisfactory. The acceptance of *Hardouinia* necessitates the adoption of *Australanthus*. Herr Bittner seems to ally it most closely to *Hardouinia*. I am, however, still inclined to consider it as nearly related to *Cassidulus*, with which it agrees in its ethmolysian, or almost ethmolysian apical system, in the characters of the tuberculation of the actinal surface, in the subpetaloid ambulacra, and in the forward position of the anus. These seem more important characters than the large size of the species, and the depression of the peristome. Herr Bittner suggests that the division may be only sub-generic, and as a sub-genus of *Cassidulus* I propose to adopt it.

Fam. Spatangidæ.

Schizaster, sp.

Mr. H. P. Woodward's collection from Champion's Bay includes an internal cast of an Echinoid which is in all probability a *Schizaster*. In the absence, however, of any knowledge in regard to the fascioles, the generic position cannot be determined. Mr. Woodward[1] quotes the genus *Brissus* as occurring at Shark's Bay; otherwise this is the only Spatangoid from Western Australia.

Macropneustes decipiens (Tate).

That the Spatangoidea will always be grouped mainly by the arrangement of the fascioles is not at present likely to be questioned; but it is attended with the disadvantange that when species are described from but one or two specimens, their true generic position is very likely to be uncertain. Thus the species now known as *Sarsella forbesi* (Woodw. and Dunc.) thrice changed genus owing to better specimens demonstrating the presence of fascioles not shown in those previously described. In Prof. Tate's admirable Bibliography and Revised List of Australian Echinoids, he has

[1] H. P. Woodward, Western Australia, Annual General Report for 1890, p. 14. (Perth, 1891.)

shown that a similar change must be made in the position of a species which I referred to *Pericosmus compressus* (Duncan). He removes the specimen figured to *Eupatagus* on the ground of the tubercles on the abactinal surface; but this alone would be insufficient to necessitate its removal. I have, however, worked away the matrix covering the subanal area of the type-specimen, a risk to which I did not previously care to subject it. This shows that a subanal fasciole is present; the species must therefore be removed from the Prymnadete to the Prymnadesmian group. But in *Eupatagus* the species has not reached its final resting place, for in that genus the petals of the paired ambulacra are broad and flush, with large poriferous zones: in this species they are long, narrow, and depressed, and the pores are small, and not so strikingly dissimilar in size. It therefore belongs to the genus *Macropneustes*, which Prof. Duncan regarded as only a sub-genus of *Eupatagus*,[1] but which most authors keep as a quite distinct genus. The synonymy of this species will therefore be:

Pericosmus compressus, Dunc. *non* M'Coy. Gregory, Geol. Mag. 1890, p. 485, Pl. XIV. Fig. 1.
Eupatagus decipiens, Tate. Trans. Roy. Soc. S. Australia, vol. xiv. p. 282.
Macropneustes decipiens, Tate. Gregory.

DESCRIPTION OF PLATE XII.

Fig. 1.—*Laganum decagonale* (Less.) var. *rictum*, n. var. Shark's Bay, W. Australia. Figs. 1*a*. Abactinal side; 1*b*. Actinal side; 1*c*. Lateral view. Nat. size.

Figs. 2, 3 and 4.—*Cassidulus florescens*, n. sp. Middle Murravian. Fyan's Ford, near Geelong. Figs. 2*a*. Abactinal side; 2*b*. Actinal side; 2*c*. Lateral view: each × 2 dia.

Fig. 3*a*.—Abactinal side; 3*b*. Actinal side; 3*c*. Lateral view; 3*d*. Apical system: × 4 dia. Fig. 4.—Posterior view of another specimen: × 2 dia.

II.—On Liassic Sections near Bridport, Dorsetshire.

By John Francis Walker, M.A., F.G.S.

During my visits to West Bay, Bridport, in the years 1887 and 1888, I was able to examine the following inland sections of the Lias Junction Bed, and I communicated a paper to the British Association at Leeds in 1890; an abstract of this paper appeared in the Report. I had hoped to obtain more evidence of the nature of this deposit, but, unfortunately, last year, 1891, I found that the working of the Allington brickfield was being abandoned, and that it was difficult to further work the roadside cuttings without doing considerable damage. I therefore think it better to lay before the readers of the Geological Magazine my notes on this deposit, which I shall be able to show is variable in different sections, due to the amount of denudation which has taken place.

Several notices of the Junction bed have been written, but chiefly with reference to the sea-coast section. I will only refer to those required for discussion in this paper.

In the Quarterly Journal of the Geological Society, 1863, Mr. Day gave an account of the junction bed of the Upper and Middle

[1] M. Cotteau has recently given admirable figures of the type species *Macropneustes deshayesi*, Ag., Pal. Franç. Échinides Eocènes, pl. xxxi–xxxiii.

Lias on the sea-coast of Dorset. He describes it as "a remarkable band of stone, the lower part of which is in a great part a conglomerate, the pebbles being imbedded in a more or less ferruginous matrix with Oolitic granules." "In places, however, this bed assumes more the appearance of the Marlstone of other districts." The higher part is composed of thin beds of a hard, dense, almost chert-like, limestone, separated by thin laminæ of yellow ochreous clay, the whole being consolidated into one block; he states the thickness of the Marlstone and Limestone to be from two to three feet; that *Ammonites serpentinum* (*falciferum*) occurs in the lower part of the Upper Lias Limestone in some abundance, though badly preserved, and considers denudation of that bed had taken place.

This junction bed is also well described by H. B. Woodward, in his valuable work on "The Geology of England and Wales," "as a pink and cream-coloured limestone in the upper part, and a brown nodular marlstone below." Mr. S. S. Buckman, in a paper (Quart. Journ. Geol. Soc. 1890), refers to this rock, and states the limestone of the fallen blocks is in two layers, *Hildoceras bifrons* being dominant in the upper layer, and *Harpoceras falciferum* in the lower.

I have very little to add to these remarks on the sea-coast section, except that I have found blocks of the limestone four feet thick, generally cream-coloured in the upper and pink in the lower part, and that the sandy part is sometimes a conglomerate, and at other times a marlstone, as pointed out by Mr. Day. Blocks of this stone were collected from Down Cliffs and under Thorncombe Beacon, and used for building walls at Chideock, hence the fossils in old collections are labelled from Chideock.

The following are the Inland Sections which I have examined:—

I.—The roadside cutting at North Allington below the brickfield (1887):

		ft.	in.
A.	Clay, worked as a brickfield.		
B. (1)	White limestone	0	8
(2)	Clay	0	1
(3)	Brown and red limestone	0	11
(4)	Marlstone	0	8
C.	Sandy Clay	3	2
D.	Brown sandy limestone, blue in centre	0	5
E.	About two yards of sandy marl, partly covered with grass and roadside scrapings	6	0
F.	Brown friable sandstone	2	1

From the brickfield I obtained a block of stone, probably from the next stone band, which contained *Monotis inæquivalvis* and *Rhynchonella amalthei*.

The fossils which I have been able to determine from bed D are:

Rhynchonella tetrahedra, var. *Northamptonensis*.
Rhynchonella furcillata.
Waldheimia (*Zeilleria*) *perforata*, var.
Spiriferina pinguis.
Monotis inæquivalvis.
Pecten, sp.
Plicatula spinosa.
Pholodomya ambigua.
Pleuromya costata.
Belemnites paxillosus.

About one foot of the clay above this stone band contained the same fossils, and others which were too imperfect to name.

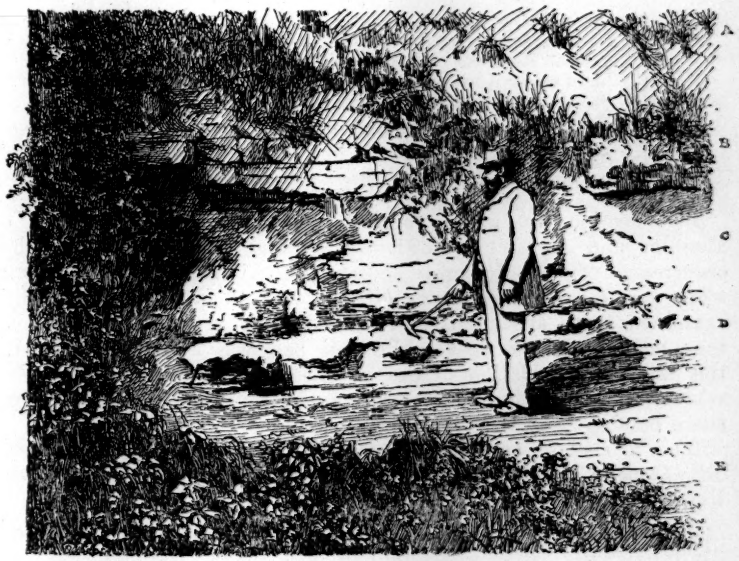
This bed, D, probably belongs to the upper part of the Margaritatus zone.

It was impossible to work the pink and white rocks without destroying the bank on the roadside. I was unable to obtain any fossils from the brick-clay, although I told the workmen to search for them.

The chief interest of this section is that it shows a division in the stone band, and the position of the stone band D, which contains fossils that correspond to those found on the beach in a bed of brown sandy limestone.

II. In the field opposite, which was formerly worked as a brick-field, about 15 feet of brick-clay having been removed, the following section was exposed in 1888:

		ft.	in.
A. 1.	Surface soil	0	6
2.	Clay	2	10
3.	Ferruginous marl	0	3
B. 1.	Hard limestone	1	2
2.	Marlstone	0	6
C.	Sandy clay	0	6



Section at North Allington.
(From a photograph by Mrs. E. Penton.)

I obtained the following fossils from the loose blocks of marlstone:

Rhynchonella tetrahedra.
—— *serrata.*
—— *egretta*, var.
Terebratula punctata.
Spirifer ina rostrata.
Lima, sp.
Pleuromya costata.
Cryptænia expansa.
Belemnites paxillosus.

From the lower part of the hard limestone:

Rhynchonella Bouchardi.	*Ammonites* (*Hildoceras*) *bifrons.*
Waldheimia Lycetti	*Ammonites* (*Harpoceras*) *falciferum.*

And from the Upper part *Ammonites* (*Harpoceras*) *striatulum.* Mr. H. B. Woodward also records the occurrence of this Ammonite at Allington. It will be observed that the thickness of the bed B, including the marlstone, is only 1 foot 8 inches; in the section on the other side of the road it was 2 feet 4 inches, but the bed was probably thicker in some parts of this field, as I was informed that large quantities of stone were formerly obtained from it. The Marlstone fossils in this quarry were not found mixed with Upper Lias species in the same blocks of stone.

There is an indication of the presence of the Jurense zone in the upper part of the stone band.

Some peculiar forms of Brachiopoda occur in this quarry which require careful study.

III.—A deep cutting in Shoots Lane, Symondsbury, which was overgrown with vegetation, appeared to show the following sections:

	ft.	in.
(1) Sandy clay, overgrown, about...	10	0
(2) Dark ferruginous rock containing nodules and worn small specimens of *Ammonites* (*Hildoceras*) *bifrons*	0	5
(3) Light coloured stone	1	2
(4) Stone band (in three divisions)	1	6
(5) Marlstone	2	11
(6) Soft brown sandy stone	0	4
(7) Clay	0	6
(8) Overgrown	3	4

This section was again carefully measured by the Rev. J. L. Templer and myself in 1891. The chief peculiarities are—the occurrences of a conglomerate bed above the junction bed containing worn specimens of *Hildoceras bifrons*. Mr. S. S. Buckman kindly examined these specimens and agreed with my determination of them; this indicates that while this bed was being deposited in this section, denudation of the bifrons zone was taking place elsewhere. The junction bed appears to be represented by the three stone bands.

The marlstone contains *Rhynchonella serrata* in its upper, and *Rhynchonella tetrahedra* in its lowest part. Should this lane ever have to be widened no doubt many fossils will be found.

IV.—In the year 1887, the supply of stone from the Forest Marble having fallen short, a hole was made on the roadside of Shipton Long Lane, Bothenhampton, for road metal; unfortunately the police ordered this hole to be filled up before it had time to weather. It was about $5\frac{1}{2}$ yds. long, 3 yds. wide, and $6\frac{1}{2}$ ft. deep, which is equal to between 30 and 40 cubic yards of solid stone. It was all in one block and required 11 lbs. of gunpowder to blast it. From information obtained from the workman, by measurement of the blocks and many days' work at breaking them and carefully collecting the fossils, the following appeared to have been the section:—

	ft.	in.
(1) Unfossiliferous sandstone, about	0	4
(2) Top band of white stone	1	0
(3) Brown stone	1	0
(4) Brown conglomerate often with pink stone at the base	2	0
(5) Marlstone	1	0
(6) Blue unfossiliferous limestone	0	8

The above may be regarded as the average thickness of the bed.

The *blue limestone*, which the workman said was the base of the rock, was coated on its lower surface with calcite, and rested on sand.

The *Marlstone* was of the usual brown colour, becoming yellow and more friable in some blocks, but towards its upper part was redder, and appeared in many blocks to gradually pass into the pink limestone which generally occurred at the base of the conglomerate; the pink rock appeared to have been slowly deposited on the surface of the marlstone.

The *Conglomerate* bed varied in colour, being mostly brown, differing from the marlstone in being harder and of a lighter colour; in some parts it had a greenish tint; its lower part was a pink rock with Oolitic grains, in the upper part it contained nodules, and some of the blocks were perforated by boring shells. Above this was a light brown stone which joined the hard cream-coloured stone. The top bed was an unfossiliferous sandstone which reached the surface of the road.

The fossils, especially the Brachiopoda, were carefully collected by breaking the blocks; after a few days' work we were able to sort the blocks, and to tell what species they would contain. No specimens of Vertebrata were obtained. Many specimens of Gasteropoda were found, but owing to the hardness of the rock it was impossible to extract them entire. It also contained several species of *Pecten*, *Lima*, etc.

The Marlstone contained in its lower part:

Spiriferina rostrata.
Rhynchonella tetrahedra, Sow.
——— *egretta*? E. Desl.
Terebratula punctata, Sow.
——— sp. (*T. Jauberti*)? E. Desl.
——— *Edwardsii*, Dav.
Waldheimia (*Aulacothyris*) *resupinata*, Sow.
——— (*Aulacothyris*) *Moorei*, Desl.
——— (*Zeilleria*) *indentata*, var. Sow.
——— (*Zeilleria*), sp.
——— *subnumismalis*, Dav.

Along with *Ammonites* (*Amaltheus*) *spinatus*.

In the upper part of the Marlstone *Rhychonella serrata*, Sow.

Remarks.—It will be noticed that *Rhyn. serrata* occurs in the upper part of the Marlstone. It is stated by Mr. C. Moore to occupy the same position near Ilminster; this species, generally a rare fossil, was very abundant. I obtained nearly 200 specimens, more or less perfect, in a bed about three inches thick. It will be remembered that the area of the pit was about seventeen square yards. Some varieties of *Rhyn. serrata* show that it is related to *Rhyn. quinqueplicata*, Quenstedt.

Rhynchonella tetrahedra was not common, and was a fine ribbed variety.

Rhynchonella egretta? This species is referred by Davidson in

his Ool. Suppt. to *Rhyn. egretta*; but some specimens closely resemble E. Deslongchamp's figure of *Rhynchonella fallax*; it will have to be compared with the French specimens, which are very difficult to obtain; it is a very variable form.

Mr. Day gives *Rhynchonella acuta* from the Down cliffs, where I have also found it. I did not see a fragment of it in this section. There is a remarkable form of *Terebratula*, of which I obtained three perfect specimens, probably a variety of *T. Jauberti*, E. Desl.

Waldheimia (Aulacothyris) resupinata does not appear to have been found by Mr. Day, as he states it is altogether absent in the sea-coast section. It is rare at Bothenhampton; I only obtained eight specimens, two of which were imperfect; it agrees with the South Petherton form.

Waldheimia (Aulacothyris) Moorei, one typical specimen.
Waldheimia (Zeilleria) indentata, rare, a wide variety.
Waldheimia (Zeilleria) sp, this may be new, it is nearest to *W. scalprata*, Quenstedt. It is a small triangular form belonging to the *W. Waterhousei* and *W. digona* group.
Waldheimia (Zeilleria) subnumismalis, rare, and not so fine as those of the coast section.

The *Conglomerate bed*, in its lower part, contained in some blocks large quantities of a *Belemnite* probably *B. paxillosus;* also worn specimens of *Ammonites* (*Harpoceras*) *falciferum*. But in many blocks the pink stone rested upon the *Rhynchonella serrata* bed, the Ammonites, which occurred very abundantly, were *Ammonites* (*Hildoceras*) *bifrons;* they are fine, large, and very well preserved specimens, although difficult to extract on account of the hardness of the matrix; they are filled with the pink rock, showing them to be of the age of that deposit.

The Brachiopoda in the pink rock were *Rhynchonella Bouchardi*, Dav.; *Rhynchonella Moorei*, Dav.; *Waldheimia* (*Zeilleria*) *Lycetti*, Dav.

Rhynchonella Bouchardi was the most abundant species, and exhibited all the varieties which occur in the same horizon near Ilminster. *Rhynchonella Moorei* was rare and small. *Waldheimia Lycetti* was of the typical form, and also resembled the Ilminster specimens. A few specimens of a variety of *Ammonites* (*Stephanoceras*) *communis*, and a large *Nautilus*, occurred sparingly in the conglomerate bed, generally of a yellowish colour.

In the brown stone over the conglomerate fine specimens of *Ammonites* (*Grammoceras*) *Thouarcense*, d'Orb., were obtained. In the hard white stone *Ammonites Germani*, d'Orb., were found in a very fine condition, and exactly resemble d'Orbigny's figures in the Paléontologie Française. This species is considered by some authors to be the same as *Ammonites hircinus* of Schlotheim. The presence of these Ammonites shows that the Jurense zone is here represented.

A curious *Rhynchonella*, character ill-defined, occurs in the brown rock, and appears to extend downwards into the conglomerate bed; some of the specimens seem to agree with *Rhynchonella jurensis*, Quenstedt, but they are generally more globose and larger shells, it may be called var. *Bothenhamptonensis*.

No specimens of marlstone Brachiopoda or Cephalopoda were found in the Conglomerate bed.

The results I arrive at are—(1) That the true Marlstone exists in the lower part of the stone band containing its characteristic Brachiopoda and *A. spinatus*; the upper part being full of *Rhynchonella serrata*, which is often overlain by a pink rock of the zone of *Rhynchonella Bouchardi*.

(2) That the conglomerate bed in the Bothenhampton section is not older than the age of *Ammonites bifrons*—the zones of *Ammonites falciferum* and *Ammonites communis* having been denuded and their worn fossils deposited in this bed. That in other localities the zone of *Am. bifrons* has been denuded.

(3) That no fossils derived from the marlstone were found in the conglomerate; and as so many fine specimens of *Rhynchonella serrata* were found, it is probable that the marlstone did not suffer denudation in this locality. We know from sections round Ilminster that *Rhynchonella serrata* is only found in the upper part of the marlstone.

(4) That the section at Bothenhampton showed that the zone of *A. jurensis* formed the upper part of the rock band.

In conclusion, I regret that want of material prevented my paper being more complete; but I must thank my Bridport friends for their kindness in affording me facilities for examining these beds, and hope that they will carefully record any excavations which may be made in this interesting deposit.

III.—Further Remarks on the Coniston Limestone.

By J. E. Marr, M.A., F.R.S., Sec.G.S.

I QUITE agree with Mr. Goodchild's statement in the July Number of the Geol. Mag. that the stratigraphy of some of the areas in which the Coniston Limestone Series is developed "presents very considerable difficulties," so much so that in the areas of Cross Fell and Settle portions of the "country might be described as consisting of a gigantic fault-breccia," and that it is necessary "to go over a large part of this faulted area again and again" in order to interpret its structure. I do not know whether Mr. Goodchild would class me amongst the "less fortunate" ones who have not been over the ground again and again; possibly I have not devoted the amount of time which he has been able to give to the study of the rocks of the Cross Fell Inlier, but it must be remembered that Prof. Nicholson, with whom I had the pleasure of working at this inlier, has returned to the ground again and again during a long course of years, whilst more recently, he and I have devoted several vacations to its study, and we have carefully compared the beds and their fossils with those of adjoining and more distant areas. Under these circumstances we are, perhaps, justified in speaking with some confidence as to the order of succession of the series; for our knowledge of adjoining regions would certainly lead us to place more reliance on the fossils of the beds than on the apparent succession of the beds themselves, where the

country partially resembles "a gigantic fault-breccia." Doubtless we have made mistakes, and shall willingly acknowledge them, when proved by ourselves or others, but proof is certainly required, and for my own part I must demur to Mr. Goodchild's "corrections" when they are only matters of personal opinion. When he publishes his evidence, if it is convincing, I will accept the "corrections," but until then I prefer our own conclusions, arrived at after considerable study of included fossils, as well as of the rocks themselves.

Mr. Goodchild chiefly comments upon our interpretation of the rocks of the Cross Fell Inlier, and adds some remarks upon my notes of the Craven area. It will be convenient to consider his comments upon each of these areas in turn.

The Bala rocks of the Cross Fell Inlier.—Mr. Goodchild states that for "field purposes" it is sufficient to divide these rocks into a lower shaley and an upper mainly calcareous series, and that "any local change from argillaceous to calcareous is, as might be expected, accompanied by a corresponding change in the fossils." If this be so, the fossil lists in our paper on "The Cross Fell Inlier" must be entirely incorrect, and our work practically worthless. Our principal aim was to show that the Coniston Limestone Series was divisible into three main groups, which I have referred to in my paper in the March Number of the GEOLOGICAL MAGAZINE as the Roman Fell, the Sleddale, and the Ashgill groups. Each of these groups contains both calcareous and argillaceous members, yet the fauna of each group differs markedly from that of the other two, whilst the calcareous and argillaceous members of each have usually many fossils in common; this will be seen by examination of our fossil lists, and I appeal to them as evidence. Not only are the fossils of the various groups different (whatever may be the lithological characters of the component beds of each), but we have shown that the faunas follow one another in an order corresponding with that observable in the equivalent beds at home and abroad. Previous experience warrants one in accepting such order in a complex district, rather than a division sufficient for "field purposes," in support of which no palæontological evidence is advanced. I may notice that in Swindale, which shows the most complete section of the Bala rocks in the Cross Fell Inlier, the actual order of succession is that which we have inferred from the fossil contents of the strata.

Mr. Goodchild believes that the Keisley limestone belongs to a higher part of his mainly "calcareous series than has been left by pre-Silurian denudation elsewhere in the area under notice." From this remark and the insertion in his table on p. 298 of an unconformity between the Silurian rocks and the Coniston Limestone Series, it would appear that he considers that there was denudation of the Coniston Limestone beds before the deposition of the Stockdale Shales. Will he give his evidence for this? He states that the Keisley Limestone is "faulted in all round," so that there can hardly be evidence at Keisley itself. The only localities we have seen in the Cross Fell area where the Skelgill Beds are shown, viz. Rundale Beck, the slopes of Dufton Pike, and the Alston Moor road,

near Melmerby, do not exhibit their relationships to the underlying series, and in the adjoining Lake District there is perfect conformity between the highest member of the Coniston Limestone series (the Ashgill Shales) and the Skelgill Beds, yet the fauna of the Ashgill Shales is not that of the Keisley Limestone, and an analysis of the forms of the latter indicates that it is distinctly on a lower horizon than the Ashgill Shales (which are found in the Cross Fell area in Swindale).

On p. 296 Mr. Goodchild states that he believes the volcanic rocks of Dufton Pike, etc., are "of pyroclastic origin everywhere in this area," but he gives no evidence for this belief, which is directly opposed to Mr. Harker's opinion of the nature of these rocks (see his Appendix to our paper on the Cross Fell Inlier), based on careful microscopic examination.

Mr. Goodchild next notices at some length the geology of the slopes of Roman Fell, but he only gives the "facts as they appear" to him. Professor Nicholson and myself have re-examined the Roman Fell rocks since the reception of Mr. Goodchild's letter containing "the friendly hint." We are willing to admit that our map of the Roman Fell country is too generalized (though it was distinctly stated that it was a sketch-map for temporary use prior to the publication of the Geological Survey Maps), but we do not see that our general conclusions are thereby affected.

There is probably a cross fault between the Dufton Shales of Hilton Beck (Mr. Goodchild's Helton Beck) and the rocks further south, causing the latter to be shifted back to the east, so that the *corona* beds should not be taken below the Hilton Shales in the course of the stream. But we cannot recognize any evidence of the intercalation of volcanic beds between two bands containing *Trematis corona*, for the volcanic rocks on the east side of the Seat seem to us to be distinctly connected with the Skiddaw Slates, and to occur on the east side of the Knock-Flagdaw fault. We did not go over this part of the ground before our paper was written, as our object was not a minute description of the sedimentary and volcanic rocks of the Skiddaw Slate Series. Whilst, therefore, I see no objection to the intercalation of volcanic rocks in the *Corona* beds (it is well known that they are intercalated on more than one horizon between different sedimentary bands of the Sleddale group), I am not convinced that such occurs on Roman Fell, though we have recently re-examined the ground for this special purpose.[1] When Mr. Goodchild remarks, however, "I more than suspect that these Helton Moor volcanic rocks are the equivalents in time of those I named the Rake Brow Series," which he has elsewhere identified with the volcanic rocks of Eycott Hill, we look

[1] Mr. Goodchild appears to have misunderstood Prof. Lapworth's information about the *Corona* beds, for Prof. Lapworth tells me he knows of no *corona* beds at Girvan, or elsewhere in Scotland. Perhaps Mr. Goodchild refers to the occurrence of two bands of *Trematis* in the Shropshire area, which Prof. Lapworth tells me he has found there. In quoting so eminent an authority as Prof. Lapworth in support of a controversial point, surely we are justified in expecting that a correct version of Prof. Lapworth's statement should be given.

forward eagerly to the publication of the evidence on which he relies, as the position of the Eycott group is one of the most important problems in Cumbrian Geology, and there are serious difficulties in the supposition that the equivalents of the Eycott Hill rocks are anywhere intercalated between beds containing the *Corona* fauna.

I may here correct one statement made in our paper on the Cross Fell Inlier. The volcanic rocks south of Lycum Sike (which are very rotten) are not the Dufton rhyolitic rocks. As they are faulted against the beds further north, their position does not, however, affect the main question as to whether volcanic rocks occur intercalated between two sets of strata containing *Trematis corona*.

The term Corona beds is objected to, because of the stated intercalation of volcanic rocks between two Corona beds, and because "*Trematis corona* occurs in the Coniston shales in several localities." I cannot see that the term is inappropriate, even if an intercalation of volcanic rocks splits them up (which we do not admit). As to the occurrence of *Corona* in the Coniston shales, we maintain that it is limited to that portion of the Coniston shales which we have termed "*Corona* beds," and shall continue to do so until definite evidence to the contrary is adduced, for the fauna of the *Corona* beds is quite different from that of higher members of the Coniston Limestone series.

Mr. Goodchild fails to see any reason why the *Corona* beds "may not be contemporaneous" with the Drygill beds of the Caldbeck Fells. A very good reason is that they only contain one fossil in common, viz. the Brachiopod *Orthis testudinaria*, which ranges through Llandeilo and Bala rocks, and that the other fossils indicate a different horizon from that occupied by the *Corona* beds.

The Craven area.—Little need be said concerning the rocks of this area. My expression of opinion as to the age of the Ingleton green slates is of little value until the evidence is published, and I should not have ventured any opinion had that of previous writers been unanimous. I have, however, at various times, both alone and in company with Prof. Nicholson, examined the rocks of the Ingleton area with a view of finding the asserted passage from the Ingleton green slates into the Coniston Limestone series, and have seen no proofs of such passage.

The Ingleton green slates, or rather the coarser bands in them, are marked by an abundance of detrital mica, a mineral which is conspicuous by its absence amongst the volcanic rocks of the Lake District. Also, whatever the Ingleton slates are, they have undergone very considerable alteration by pressure metamorphism since their deposition, a metamorphism which seems at first sight far greater than that which affects the rocks of the Coniston Limestone series. I am content to await the judgment of petrologists on this point.

The concluding paragraph of Mr. Goodchild's paper contains a truism. But whether the persistent types of the Ordovician rocks are found in Craven and within the Cross Fell Inlier is another matter. I have not dwelt on the subject because I do not consider

that the time has yet come for doing so. The work which I have done in the Lake District, both alone and with Prof. Nicholson and Mr. Harker, is merely preliminary to the elucidation of the general history of the district. Much remains to be done, both by careful microscopic study of the rocks and by examination of all the available fossil evidence, before the full history of the rocks of the Lake District and adjoining areas can be written. Mr. Goodchild has given us his interpretation, though without full discussion of the palæontological evidence.[1] Whether that interpretation is correct time alone will show, but in the meantime I shall bow down to no conclusions which are not fully supported by the evidence of the fossils, for the past history of geology shows us the errors which have arisen from misinterpretation of such evidence.

IV.—Note on a Granite Junction in the Ross of Mull.

By J. G. Goodchild, F.G.S.;
of Her Majesty's Geological Survey.

[Communicated by permission of the Director General of the Survey].

In a review of Nicholson's "Geology of Cumberland and Westmoreland," which appeared in the Geological Magazine about a quarter of a century ago, the reviewer observes, in referring to some remarks upon the Shap Granite, that this and other granites have clearly taken the place of the rocks whose position they occupy, and that, in fact, intrusive rocks can be shown, in a great majority of instances, to have replaced, rather than to have displaced, the rock masses which they invade. The reviewer's identity with a well-known Professor of Geology at one of the English Universities is sufficiently evidenced by his intimate acquaintance with the geology of British Silurian and Cambrian rocks.[2] Like much else that has emanated from the same source these particular observations were

[1] In support of this statement, I may quote a remark made by Mr. Goodchild in a paper read before the Geologists' Association on July 5th, 1889, in which he refers to the paper by Professor Nicholson and myself on the Stockdale Shales. "If we listen to our palæontologists we must classify the Graptolitic Mudstones with the Ordovicians; while, if we are guided by the physical evidence alone, then the Graptolitic Mudstones with their Ordovician fauna must go in the same group with the beds above, and be classed as Silurian." If Mr. Goodchild had studied our paper, he would have discovered that the Graptolitic Mudstones (there called the Skelgill Beds) do not contain a single Ordovician form, with the possible exception of *Climacograptus normalis*. Again, in the same paper Mr. Goodchild states that during the accumulation of the Browgill Beds "deep oceanic conditions prevailed, and the old colony of graptolites either migrated still further, or else it became completely extirpated here." A glance at our paper would have shown that two well-marked graptolitic zones occur in the Browgill Beds of the Cross Fell area, and that the graptolites of these zones are intermediate between those of the Skelgill Beds and those of the Lower Coniston Flags, which according to Mr. Goodchild "migrated from a different zoological province!" Mr. Goodchild's depressions, elevations, and migrations belong to a past generation who made much of "Colonies" and similar ingenious explanations; but it is refreshing to meet with them again, after the accurate work on Graptolites of Lapworth, Linnarsson, Tullberg, and others.

[2] A reference to the same phenomena is given in the Geological Survey Memoir on the Sedbergh District, in connection with some minette dykes there.

made a long time before geologists in general were disposed to give any serious attention to them.

Some years after the review appeared Mr. C. T. Clough, of the Geological Survey, after a long and careful series of observations in the field, came to the conclusion that the Whin Sill of Teesdale—a well-known intrusive sheet of dolerite invading the Carboniferous rocks of the North of England—replaced these rocks, and had not wedged them asunder, as it had commonly been supposed they had done. Mr. Clough's observation made it certain that the total thickness comprised between any two given upper and lower horizons in the strata, invaded by this dolerite remained, exactly the same where the Whin Sill was present as it did where it was absent, and that the strata above it and below remained, not only at their normal distance apart, but were absolutely undisturbed by the intrusive mass, whatever its thickness might happen to be. Mr. Clough argued that eruptive rocks such as these had assimilated the strata whose position they occupy at present; and he further accounted for the uniformity of composition of the dolerite where the rock invaded happened to present a wide range of chemical composition by the theory that a kind of circulation went on throughout the molten mass, whereby the rock material, as fast as it was melted up, became diffused throughout the whole mass, instead of remaining more or less localised.

Facts of the same nature as those observed by Mr. Clough had been repeatedly noted in the areas adjoining by his colleagues on the Survey; and, indeed, every competent geologist who had examined the field-evidence was entirely in accord with Mr. Clough in his conclusions.

But the cabinet geologist declined even to consider the matter—he said that it was simply impossible that an intrusive mass could replace the rocks it invaded; and he found so numerous a body to agree with him, and therefore disposed to rest content with the old view, that it was clear to those who held more advanced views that there was no chance of the subject meeting with fair discussion for a long time to come. So the matter was allowed to drop, and the steady-going section of geologists again went on their way in peace.

It is somewhat remarkable that there should be so much reluctance to accept this view, considering that the best text-books had, long before, given illustrations of this very mode of occurrence of intrusive rocks, and that the maps and sections published by the Geological Survey furnished abundant evidence pointing to the same conclusions. And it is almost inconceivable how anyone who had worked out the inter-relations of these rocks in the field, could possibly adopt any other view. Apart from any theoretical bias that a field-geologist might have in this matter, he could not very well overlook the fact that his fellow-worker, the miner, has long been aware that trap rocks very often "cut out" the coal seams or other strata with which they come into contact. This is abundantly proved in the Midland coal fields, as well as in those of the more northern parts of the kingdom, and it is a rule of very general application elsewhere.

Some important problems relating to both physics and chemistry are involved in this matter. These I could not even attempt to solve. But some little additional field evidence gained while studying the behaviour of several granite masses in Cornwall, Cumberland, and Westmoreland, in some parts of southern Scotland, and especially in the Ross of Mull, seems to me to indicate the direction in which further research, systematically carried out, may lead eventually to a correct understanding of the facts.

Around the border of the Ross of Mull, chiefly on its eastern and south-eastern margin, the granite contains unusually large quantities of included masses of rock, chiefly quartzites, greywackes, and mica schists, identical in character with that of the Highland Metamorphic Series, which the granite here penetrates. In some of the larger granite quarries, as for example, those at Camas Tuadh on the west side of Loch Làthaich, and others again near Ardalanish, these included blocks occur in such profusion as to impart to the rock the appearance of a gigantic breccia, in which the granite itself plays but the rôle of matrix. In many instances the blocks are as much as twenty or thirty feet in length, and are but little rounded, nor are they much altered beyond the stage of metamorphism observable in these rocks at points far removed from the granite. The parent masses do not appear to have undergone much shearing prior to their engulphment in the granite, so that the lines of stratification, the false bedding, and the joints of the original quartzite are distinctly traceable through the included blocks in their present position. The inclusions appear to have been detached from the parent block by means of joint planes, and, once so detached, to have quietly floated away into the molten rock, without any further change specially noteworthy.

I had occasion, a year or two ago, to follow the boundary of the granite for some miles, beginning at its clear junction with the quartzites at Ard an Daraich, and following it from that place southward. The task of laying down this boundary upon the map was by no means as simple as it is in the majority of cases. Many granites come against their enclosing rock with a perfectly well-defined junction, often narrow enough to be covered with a knife edge. But the Ross of Mull granite behaves differently. In some respects, indeed, it may be said to shade off into the metamorphic schists around. It is not intended by this that the mica schists or the quartzites showed an increasing percentage of crystalline felspar in proportion to their nearness to the granite. The microscopic structure, as well as the field evidence, showed that this case forms no exception to the now-generally recognized rule in this matter. The rocks are granulitized, as might have been expected, but no transition can be observed from the one mineral type into the other. The gradation referred to is effected by the steady increase in the proportion of the masses included within the granite, as this rock is traced outward towards its peripheral zone. Beyond that zone, where some few masses of schist and quartzite still remain more or less attached to their parent rocks, the number of granite

veins ramifying into the surrounding rock, along joints and oth divisional planes, is so large that in places it is difficult to s whether the section before one should be regarded as a mass schists traversed by granite veins or as a mass of granite with unusually large percentage of included blocks. Some of the be sections for the study of these phenomena are along the coast ne Carraig Mhòr and at Torr na Sealga. But even from the tour steamer, on its way past the south coast of the Ross of Mull, travelling from Iona to Oban, the phenomena are distinctly visib without the aid of a field-glass, so large are some of the masses schist and quartzite included within the granite.

A close examination of the junction between the granite veins a the invaded schists gives some clue to the way in which this extr ordinary *melange* of granite and metamorphic rock has been pr duced. The granite veins have evidently eaten their way into th surrounding rock, along joints or other planes of weakness, meltin the adjoining rock as they advanced, and without wedging any of apart, as the enormous pressure under which the granite was i truded might have been expected to do. Scores of example examined here and elsewhere, showed that the thickening of th wedges of molten rock, as they advanced into the strata beyond, wa accomplished by the gradual removal of the rock along the sides the wedge, the place of the rock so removed being taken by a equivalent bulk of granite, different in no respect but that of th size of its constituents from the granite nearer the central part the chief mass. In many cases the course of the granite tongue ca be followed along one joint, narrowing as it extends inward, an then turning off in a different direction along another joint, nearly or quite, up to an intrusion coming from another part. The detach ment of the blocks by means of wedges, which have eaten their wa in along the joint planes, can be traced through every stage quarrying up to complete isolation.

Once surrounded in this way by an envelope of molten rock, th extraction of the block, by the further enlargement of the wedge was no difficult matter. The mass of schist so liberated floatin upward into the main stream if the current was setting strongly i that direction; downward, if so determined by the relative specifi gravity of the molten as compared with the unmelted rock; o remaining suspended if the relative specific gravity, etc. so permitted

Probably the whole of the blocks so detached were destined soone or later to undergo complete solution, and, in that form, to underg amalgamation with the rest of the molten rock forced up from th reservoirs below. The process of diffusion in such a case may b likened to what takes place when a fragment of a more-fusible meta is introduced into a molten mass of a less-fusible metal. In th case where the two readily form an alloy the diffusion is effected completely, without stirring, or any other mechanical aid. Thi principle of diffusion seems to afford the clue to the uniformity o composition of the great majority of intrusive rocks, even where they invade rocks of very diverse composition. It may be remarked

however, that the uniformity referred to is by no means as general as it appears to be assumed is the case.

The sections in the Ross of Mull suggest that as the peripheral zones of a granite mass, gradually melted up, they may have furnished no inconsiderable proportion of the molten rock that worked up to the surface in the form of lava; although it is doubtless true that every part of the conduit contributed more or less. The peripheral contributions, being at a somewhat lower temperature, and therefore having a somewhat higher specific gravity than the molten rock nearer the middle of the ascending current, may have been carried downward for a time; but they were destined, sooner or later, to float upward with the rest.

Granite veins may thus be regarded as playing the part of capillaries in the circulatory system of the larger mass; or their function may be likened to that performed by the individual leaves of a tree, which elaborate products not used entirely by the leaves themselves, but which are destined for distribution by means of the circulatory system as nourishment for the plant as a whole.

The mode of attack of an intrusive mass is probably influenced very largely by complex inter-relations between the composition and the temperature of the invader and the rock invaded. To some extent also the results may vary in accordance with the degree of resistance to intrusion presented by the invaded mass. Where this resistance was comparatively low, as would happen in the case of intrusions taking place under small superincumbent pressure, a true laccolite would doubtless be formed. That is to say, the overlying rock might really and actually be lifted up with little or no replacement of the strata affected. But where the resistance to the enormous intrusive force was too great to be overcome by such rupture and displacement, the temperature of the peripheral zone was raised to the melting point proper to the degree of pressure exerted, and the invaded rock gave way by fusion instead of yielding by upheaval or by fracture.

In this way both of the recognized types of intrusion may find a simple explanation. Where a laccolite really does occur, that exceptional phenomenon would seem to indicate intrusion under comparatively-low superincumbent pressure and usually under such conditions as must obtain in the upper part of a volcanic cone.[1] But in the case of the majority of intrusive masses the molten rock has been forced in against greater resistance; the temperature of the invaded rock has thereby been raised to its local fusing point; the melted portions have been alloyed by circulation with the general magma; and in this way the intrusive mass would, step by step, replace the rock invaded, and would replace that rock by igneous rock of uniform composition without displacing the rock beyond in the least.

[1] It should be noted that as this upper part is that which is most readily denuded away during subsidence beneath the waves, true laccolites can be but rarely preserved in a fossil state.

V.—The Malvern Crystallines.[1]

By the Rev. A. Irving, D.Sc., B.A., F.G.S.

THE author having made an examination in the field of Malvern Crystallines, during a residence of ten weeks Malvern in the early part of the year, and having had the advant of Dr. Callaway's company over portions of the ground towards end of the time, offers here an outline of his observations and general conclusions to which his field-work has led him, reservi for the present, all details of microscopic work.

I. He agrees with Dr. Callaway[2] (with one doubtful exceptio that the whole crystalline mass, from end to end, is of igneous orig but cannot follow that observer in regarding the granites as injec into the diorites (the "syenites" of Phillips and Holl). The autho observations rather lead him to doubt if any true sequence can made out between these two rocks (using the terms in a br generic sense), the observed phenomena seeming to square bet with the view, which would regard them as segregation-products "one original unerupted magma," as was suggested more than fo years ago by the late Professor J. Phillips, F.R.S.[3] (see Mem. Ge Survey, vol. ii.); and he regards the coarse pegmatitic varieties granite (the "binary granite" of Callaway) as the *siliceous residu of the magma, after the whole (or nearly the whole) of the silicates the heavier bases had crystallized out*;[4] applying this even to su a coarsely-crystalline mass as may be seen on the top of Worcest Beacon, where one mass of individualized quartz was observed near a foot thick.[5] He bases this view on such facts as the following:

1stly. Upon the way in which the dioritic and the granitic rocks the chain graduate into one another (as in Worcester Beacon a North Hill), or alternate with one another (as in the Swinyard Hil

[1] A paper read before Section C. of the British Assoc., Edinburgh meeting, 18

[2] See the two able papers by that author in the Q.J.G.S. for 1887 and 1889.

[3] Even at that early date Phillips had as clear an idea of the differentiation a homogeneous fused mass into a heterogeneous crystalline mass as a recent writ in "Natural Science" (June, 1892), whose application of the theory of "osmo pressure" (as applied to "dilute solutions"), to help him to construct a "sequen of plutonic rocks" (a theory which can hardly be said as yet to have establish itself in Chemical Physics) seems rather strained. Durocher's principle of "liqu tion" is probably nearer the truth. But as different physical conditions (chief temperature and pressure) determine the allotropes of one and the same chemic body, so they undoubtedly are large determining factors of the order of crystalliza tion of minerals out of a given magma, and the consequent composition a critical temperature of the residual magma at any stage of rock geneisis. It is hard necessary to state, that with much that is contained in the article referred to th present writer entirely agrees.

[4] The fact that most of the felspar in the Malverns turns out to be plagioclas (carrying over many of the "syenites" of the earlier writers to the diorites) is i favour of this view; since "soda, weight for weight, fluxes more than potash; and "lime may produce, with a great number of infusible, or slightly fusibl silicates, compounds which melt easily." (Percy, "Fuel, etc." London, Joh Murray, 1875, pp. 73, 75).

[5] Individual hornblendes, comparable for size with these, were not met with i the field, but some may be seen in the Museum at Malvern College.

it being a rare thing to find either the granite or the diorite displaying anything like a dyke-relation to the other, the few cases in which there is a semblance of such a relation being perhaps best explained by *internal stresses* setting up slight local shearing-movements (under the influence of gravitation and perhaps other forces) along planes or zones of weakness in the rock-mass caused by *unequal contraction of the more acid and the more basic portions during congelation.* (The gradation from diorite through a biotite- or hornblendic- granite to a very coarse pegmatite may be seen at West Malvern near the church.)

2ndly. Upon the way in which the felspathic and the quartzo-felspathic veins ramify in all directions through the diorite, as pointed out long ago by Phillips (*loc. cit.*). (North Hill, and the quarries at North Malvern and at the Lower Wych, afford excellent examples of this relation.)

3rdly. Upon the fact that very often the hornblendic rock is observed to be more completely basic in immediate contiguity with the felspathic veins; whereas we should expect the contrary to be the case, if the latter were injected veins.

4thly. Upon the very significant fact, that the quartzo-felspathic veins, even when less than an inch in thickness, have a coarsely-granular texture, this being so manifest macroscopically (especially when the rock is slightly weathered) as to make it impossible to believe that they crystallized in contact with a colder rock after injection into it.

On the other hand, we may perhaps understand the facts observed, with the light thrown upon them from the *rate of solidification of basic and acid slags* observed years ago by Macfarlane.[1] The observed field-relations of the Granite-diorite series at Malvern seem to accord with his observations on slags, which warrant the hypothesis that the more *basic* portions of the magma solidified more rapidly and at a higher temperature than the more acid veins, the latter continuing for a long time in a viscous condition,[2] in which condition they would of course act as "lubricating material" between the already solidified basic portions, allowing easy relative movement of those parts, as the result of internal stresses, set up within the mass. It is possible that in this way many cases of schistosity observable in some of the felspathic veins may have been produced at that stage of their history. The author feels the more justified in putting forward this view, from the differentiation of structure

[1] See "Canadian Naturalist" for 1864, quoted by the present writer in "Met. of Rocks," page 70 (footnote), "On the Origin of Eruptive and Primary Rocks," by T. Macfarlane, now chief Analyst to the Dominion Government. It is instructive to note the revival of the ideas of Macfarlane and Naumann in the "New Geology," as the fogs of "regional dynamic metamorphism" clear away.

[2] What the late Dr. Percy called "pasty fusion." See "Fuel, etc.," page 63. Macfarlane's observations of the wide range of temperatures, which holds good for this condition of "pasty fusion" in *acid* slags, extending both above and below the temperatures at which the more limpid *basic* slags rapidly congeal, explains how in some cases thin felspathic veins appear to fill fissures in the already solidified, but still hot, amphibolitic rock-masses. In this sense some of them may, perhaps, be regarded as "injection-veins." But they are a minor feature in the Malverns.

and composition of parts of the same igneous intrusive ma observed years ago by Allport in some dolerites; from the mo remarkable differentiation of parts of the same dyke in continuo cross-sections described by Dr. Andrew C. Lawson, of the Canadi Survey, in a paper which the author received last year from th distinguished geologist (American Geologist, March, 1891, on t "Petrographical Differentiation of certain Dykes in the Rainy Lak Region"); from known cases of the derivation of basic and ac rocks from the same magma in well explored volcanic *foci*;[1] ar from the fact that he has himself found the Bronsill intrusiv dolerite (near Eastnor) pass into an almost pure pegmatite in or part. Even where such a rock occurs as kersantite or mica-diorit there is not always evidence to point to contact-alteration as th origin of the mica. It seems easier to regard this rather as a *primar* mineral constituent of the rock in places[2] (mica taking the place the hornblende of the prevalent diorite); for in one place (We Malvern) the kersantite seemed to vein the adjacent rock, while i the greatest exposure of kersantite seen in the Malvern Range it wa intersected by basic dykes, unmistakably intrusive, and no othe acid rock was exposed. This was in the quarry at the north-we corner of Swinyard Hill, where no progressive alteration coul be observed macroscopically in the kersantite as it approached th basic dykes.

II. The only *intrusive rocks* the author has been able to recogni from end to end of the chain are (a) *felsites*, becoming at time quartz-porphyry; and (b) *dolerites* of different varieties, includin diabases.

The *felsitic dykes* contain occasionally inclusions of a basic roc (probably diorite altered by contact), and are very well seen a North Malvern, at Gold Hill north of the Wych (porphyritic) about Wind's Point, in the Raggedstone Hill (as described b Dr. Callaway), and in other localities.

The *dolerites* are much more common, and enter much mor largely into the geotectonic structure of the range, being met wit from end to end of it. They are often aphanitic in texture, an in a great proportion of instances have undergone in part intens crushing in the subsequent mountain-building stage of the histor of the region, with re-cementation into a solid breccia. Many of the boldest crags consist of these rocks (North Hill and i Rushy Valley above Great Malvern). In all such cases a specia point was made by the author (in the light of experience gained in former years in the Eifel, in the Bozen country, in the Neapolitan region, in Charnwood Forest and in Snowdonia) of searching fo evidence of *tuffs*, but without finding any, although one or tw

[1] See in particular the very instructive instances described by Credner (*Element der Geologie*, sixth edition, page 587) at Predazzo and Monzoni, in the Italian Tyrol.

[2] To contend that the mica of kersantite is always a *secondary* mineral would land us in the difficulty of having to recognise it as such (by "endogenous" alteration) in cases where kersantite occurs as an *intrusive* rock, as for examples in the Devonian and Carboniferous of the Hartz region, and in the Culm of Lower Silesia (Credner *op. cit.* pp. 464, 500).

doubtful cases are mentioned by Mr. Rutley.[1] He considers, therefore, that evidence is wanting of any *volcanic rocks*[2] (*stricto sensu*) in the Malvern Chain, except those of a later date recognized as Pebidian on the flank of the Hereford Beacon. In some parts of the range a rough parallelism can be observed between the dykes, and the prevalent trend seems to be N.W. and S.E.; but there are many exceptions to the rule, and they have had very little to do with determining the present contours of the hill-flanks.[3] In some cases they have acquired a slabby structure, as if from roughly parallel divisional planes produced by shrinkage during the final stage of their solidification. Their intrusive relation to the *Granite-diorite* series seems to be established by (i.) their dyke-like form with boundary-walls often extremely well-defined;[4] (ii.) the inclusion in them of masses of the older series (such inclusions consisting of granite, or of diorite, or of diorite veined with felspathic material); and in one instance (in an old quarry at North Malvern at the extreme end of the range) a large inclusion of hornblendic rock is seen to be itself previously intersected by felsitic veins, which are truncated off abruptly at the boundary of the enclosed mass,[5] pointing to an earlier date for the intrusion of the felsite. The enveloping dolerite itself has here acquired a slabby curvature, as if from the resistance of the included mass producing internal stresses in the dolerite during its solidification. North Hill, Worcester Beacon (near the summit and in the higher parts of Rushy Valley on its lower eastern flank), the quarry at the N.W. corner of Swinyard Hill, the Gullet Quarry, the Wych Road, and St. Anne's Well, are some localities, where this dyke-relation seems clear. These intrusive masses appear to be portions of a deeper magma, which (first the acid, then the more basic portions) simply *flowed into great fissures of the Granite-diorite series*, as the

[1] "On the Rocks of the Malvern Hills," Q.J.G.S. August, 1887.

[2] The extended sense in which this term has been lately applied by Sir A. Geikie, in his two Presidential Addresses to the Geological Society, is much to be deprecated, from the confusion of thought it is likely to engender and the violence done to the literature of petrography. The term "volcanic" is used as equivalent to "igneous," and the term "eruptive" is made to do duty for "plutonic." In this way the Lewisian gneiss is included in his volcanic series (1891 Address, p. 32). Introduce the pressure of the Archæan atmosphere, and all the facts on p. 33 tell the other way. To speak of such rocks as tourmaline-granite as parts of a great "eruptive-stock," as Credner (*loc. cit.*) does of the more deep-seated but unerupted igneous masses at Predazzo and Monzoni, is one thing; but to take the great Archæan gneiss series by themselves and label them "eruptive" or "volcanic," when there is no evidence whatever of lava-flows or tuffs to connect their history with what is usually understood by *vulcanicity*, is another thing altogether.

[3] Dr. Holl seems to have generalized rather hastily on this point. See Q.J.G.S. vol. xxi. pp. 72, *et seq.* "On the Geological Structure of the Malvern Hills, etc."

[4] Excellent examples may be cited from the Worcester Beacon and its flanks. One bold crag high up the south flank of Rushy Valley is half granite, half dolerite, the latter much crushed and brecciated, with a trace of cleavage here and there. The junction is unmistakable both on the north-looking vertical face of the crag and on the ground-plan above.

[5] In this and some other cases the included mass seems not improbably to have fallen into the later and more basic magma, and to have floated off after the manner of some of the masses described by Dr. A. C. Lawson in the Rainy Lake Region.

latter was dislocated subsequently to its consolidation. Everything seen goes to show that these intrusive masses were *altogether passive as regards the earth-movements*, which have brought the Malvern crystallines into their present position. The author's observations on this point thus agree with conclusions put forward during the last two decades by Doctors Suess and Heim.[1] The researches of Dr. Callaway and Mr. Rutley make it probable that some of these basic intrusive dykes, which look so much like dolerites under the hammer and the lens, are really fine-textured diorites or epidiorites; but this does not materially affect the view put forward here as to the part they play in the geotectonic structure of the Malvern Chain.[2]

III. *Structural Phenomena.*—These seem to fall for the most part into five categories:—

(i.) *Diorite-gneiss* (rocks composed of the materials of diorite with a gneissic arrangement).
(ii.) *Gneissose (flaserig) structures.*
(iii.) *Phyllolithic rocks* approximating in various degrees to the character of a true schist.
(iv.) *Shear-planes* (often highly slickensided).
(v.) *Crush-planes and zones.*

A few remarks, as brief as may be, are called for on each of these.

(i.) *Diorite-gneiss.*—The most perfect examples of this were met with at North Malvern. The rock here is for the most part a massive diorite, but occasionally the parallel veining of the felspathic and hornblendic constituents is very marked, in parts of the rock which do not appear to have undergone any deformation by dynamic action. The veins are often a mere fraction of an inch in thickness, and the veining has no relation whatever to the shear-planes which intersect the rock. In other places the veined structure has undergone some deformation, giving the rock a more schistose structure, the veins being apparently stretched out, the felspathic particles some-

[1] Since this was penned "Suess-réchauffé" has been largely served up for the delectation and mental nutriment of Sections C and E, by the respective presidents of those sections. The present writer can hardly find fault with Professors J. Geikie and Lapworth for giving such powerful expression to ideas which have haunted him for years past, as the references in his "Met. of Rocks" to Suess and Heim plainly show. But they can hardly lay claim to *novelty*, seeing that the "Antlitz der Erde" was outlined in (the closing chapters of) Suess's "Entstehung der Alpen" (Vienna, 1875). Lapworth's conception of the "wave and trough" deformation of the rind, following Suess and Heim, implies a *yielding under-zone in the lithosphere*, which he will find hard, with Dissipation of Energy, to harmonize with the Huttonian uniformitarian doctrine, and will act as an "aqua regia" upon the gold of his "wedding-ring," and the blessed union symbolized by it. And the recognition of the *passivity of igneous inflows and outflows* and their location generally along the *concave* side of the asymmetrical mountain-wave disposes once and for all of the fiction of the "roots of the mountains," of a veteran writer on physical questions, even as it has made it impossible for the present writer to accept it. (See in particular Heim's *Mechanismus der Gebirgs-bildung*, Bd. ii., p. 178; Basel, 1878.)

[2] Those who will be at the trouble to consult the author's "Metamorphism of Rocks" (pp. 53–55) may see that on general grounds we may recognize the presence of traces of highly superheated H_2O as a potent factor in differentiating the amphibolitic rocks from the felsitic and pyroxenic magma which subsequently filled up the fissures in them. This is thoroughly borne out by Dr. Kroutschoff's synthesis of hornblende (see GEOL. MAG. 1891, p. 303).

what rounded (as the quartzes are known to be in quartz-porphyry from abrasion in a viscous "flowing" magma), and arranged in linear series, reminding one of a loose string of beads. The rock has at the same time become distinctly fissile under the hammer in the direction of the foliation.[1] Kneading-out or stretching-out rather than crushing-out seems to have been the order of things in such cases. This description applies to the "shear-zones" of Dr. Callaway. Good examples are seen in the Gullet Quarry (south end of Swinyard Hill), in the quarry south of the Wych Pass (where larger masses of felspathic material are drawn out into slabs), in all the three quarries at West Malvern,[2] and at North Malvern above the reservoir, where they bear no relation whatever in their orientation to the crush-planes exposed in the quarries below. The differential movement in such cases, *so far as each plane of shear is concerned*, seems to have been but slight; no more, in fact, than could be referred to unequal contraction of adjacent portions of the rock-mass during the later stage of consolidation. In some cases, as in the quarry above the church at West Malvern, such a foliated zone occurs on both sides of a divisional plane, as if the shearing had been produced by sliding along a shrinkage-crack, while the mass on either side of it (in this case a very felspathic variety of diorite or a biotite-granite) was still in a pasty condition.

Diorite-gneiss would seem to result from the segregation of minerals in the course of their development; and its conversion locally into "diorite-schist" (amphibolite) to *differential movement prior to final consolidation of the materials.* The idea suggested itself to the author when Dr. Callaway's paper was read at the Geological Society in 1889, and certainly seems to work very well in the field.

(ii.) *Gneissose (flaserig) structures.* This kind of alternation of more basic and more acid layers, giving the rock at times a very stratiform character, may be most easily explained as due to original veining, accompanied possibly in some instances by some differential movement prior to complete solidification. This simulation of bedding is most conspicuous about the central parts of the range, as at Wind's Point, in the Hereford Beacon, and in the quarries by the roadside at Little Malvern; but at the latter place the rock again puts on its more massive structure in the quarry half-way up the flank of the hill.[3] On the ridge of Swinyard's Hill also this slabby structure of alternating hornblendic and felspathic layers may be observed. Various degrees of deformation of these layers (especially

[1] It appears to have become a rock of the Amphibolite-type of continental writers (resp. "felspar-amphibolite" or "diorite-schist"). See Credner (*op. cit.* p. 111), also Kalkowsky, "Elemente der Lithologie" (Heidelberg, 1886), p. 209.

[2] At the north end of the village is a disused quarry, which does not seem to be noticed by recent writers. In a certain broad zone of the rock exposed there, the structure here under consideration is splendidly shown.

[3] Here, however, the diorite is intersected by felsite dykes. It is interesting to note this injection of felsite into the more banded and more massive portions of the same broad orographic zone of the Malvern chain, pointing to community of age in the two. It is curious also to see how this was overlooked by earlier writers, when they wrote so confidently about these gneissose portions of the crystalline *massif* as bedded sedimentaries. (See *e.g.* Symonds "Old Stones," pp. 18, 19.)

those of a more basic character) may be seen; and in places—as in Rushy Valley above Great Malvern, and in the Raggedstone quarry—the basic minerals have been more completely pulverised and rendered incoherent by the grinding action to which they have been subjected between the harder and more rigid felspathic masses. This was observed to be especially the case where the stratiform structure of the rock had suffered considerable contortion against massive intrusive dykes of basic rock,[1] and is, therefore, one of the phases of deformation which may be with some certainty referred to dynamic action since the consolidation of the whole mass. Where this seems to have been excessive, some mica has made its appearance in the felspathic distorted bands of the rock, probably at the expense of the original felspar.

(iii.) *Phyllolithic rocks* approximating in various degrees to true schists. These seem to be very local in their development.

(a.) *Hornblende-schists.* In some of these the deformation of the hornblende crystals is the chief feature; and it seems probable that this mineral deformation, without any conspicuous shearing-movement, has occurred since the mass became fairly solid, and as a result of *static* rather than dynamic forces. It seems to occur in the more massive portions of the diorite, even where the rock-mass as a whole assumes a stratiform character, as in the large quarry at Wind's Point and in the quarries at Little Malvern. Something of this sort of crumpling was observed also in the more basic rocks extensively quarried at the Hollybush Pass.

In other cases distinct parallel planes of foliation, with marked differentiation of the minerals are seen, and lenticular plates of quartz are common, sometimes a quarter of an inch or more in thickness. This structure is well seen in the larger of the two old quarries at the south end of Raggedstone Hill, in the crags on the ridge south of the Wych, and in the crags near the ridge nearly half a mile north of the Wych above Rock Villa. Observations in the field make it difficult to resist the conclusion that these local developments of an apparently perfect schist are directly related to the greater resisting-power of the already consolidated felspathic masses contiguous to them, with which, in every case, so far as the writer's observations go, they are in close relation, either alternating with such masses (as in the crags mentioned), or the schistosity (as in the Raggedstone Quarry) being most definite and perfect in close contiguity with massive quartzo-felspathic veins, and gradually dying away into the undeformed hornblendic mass. The deficiency of felspar—if not the entire absence of that mineral—seems to be accounted for by its previous concentration in the adjoining felspathic veins; while the flattening out of the quartz has taken place in such

[1] On the top of the crag referred to (*Ante* p. 454) on the south side of Rushy Valley the granite becomes flaggy close to the junction-planes, as if from shearing, while the dolerite on the other side of the junction-plane is crushed to a breccia. In another crag lower down the gneissose granite is bent round against a massive dolerite. Good examples of this gneissose structure may be seen by the footpath, halfway up the hill.

a way as to make it probable that, when the deformation began, that mineral was in the colloid state, owing to the concentration in it of the traces of water present in the original magma, as the anhydrous minerals separated out.[1] This deformation probably occurred when the whole rock mass was still at a fairly high temperature; and, that the present structure resulted from pressure acting upon a mass, in which the minerals were already differentiated in the manner indicated prior to solidification, and *not causing that differentiation*, seems the more probable from the fact that traces of residual felspar are often seen in these quartz-veins. It is, at least, as easy to explain the observed facts in this way as to suppose that pressure has brought about a deformation of original felspar, and led to the removal of its bases with the separation-out of quartz. The same sort of flattening-out (in pancake fashion) of the quartz in masses of considerable size (several inches long) was observed in some cases of the more felspathic masses, where the mass, as a whole, has been squeezed into a dyke-like form. A notable example of this was seen in the Dingle Quarry at West Malvern.

[The retention at high temperatures of the water of hydrated silica by sufficient pressure acting hydrostatically is only another example of the same principle as we have learned to recognize in the fusion of limestone, without dissociation of the CO_2 from the CaO, as a necessary stage of marmorization. If P represent the pressure required to prevent dehydration of colloid silica or dissociation of Ca CO_3 in any given case (at high temperature), it is easy to see that P may be the sum of—

the pressure due to its depth in the lithosphere (p_1),
the pressure due to the depth of the hydrosphere, if any (p_2), and
the pressure due to the supernatant atmosphere (p_3).

The equation $P = p_1 + p_2 + p_3$ is therefore true in all cases; but with recession in time p_2 and p_3 would merge into one another, p_2 diminishing, and p_3 increasing, as we approached the "pre-oceanic" conditions of early Archæan time in the history of the lithosphere, and even so much of p_1 being added to the compound hydro-atmospheric term, that the depth in the lithosphere necessary to furnish P would not be very great. That is to say, the requisite pressure might be found at or near the surface of the lithosphere.

[1] The author has discussed this fluxing-action of *traces* of H_2O (and perhaps other oxides) elsewhere. (See "Met. of Rocks," pp. 48–50; also p. 86, where the case of quartz in granite is considered.) It may be as well to remind geologists that "fluxing-action" may display itself in *retarding congelation* of mineral matter as the temperature of a liquid or viscous mass falls, just as much as in hastening the fusion of a solid mass. These are only two aspects of the same law. The proportions in which SiO_2 may hold H_2O in combination, extend over a very wide range; and it is almost certain that the critical temperature of hydrated silica is lowered in some proportionate relation to the amount of H_2O present. Compare the results of the Author's own experiments, given in App. i. note C of his original thesis put forward in January, 1888 (p. 100 of the published work). It is possible that slight hydration of SiO_2 at depths might exist even at a red heat, pressure preventing the escape of water (as in Dr. Kroutschoff's recent experiments). Dr. Callaway has come near the idea on p. 500 of vol. xlv. of Q.J.G.S., though he seems to mistake this fluxing action of water for "solution."

This is "uniformity" indeed, but quite a different thing from the "uniformitarianism" of the Hutton-Lyell school.]

(b.) *Sericite-schists.*—The writer has adopted this term from a letter written to him by Dr. Callaway for the satiny, distinctly-foliated, and contorted rock, which is exposed on the western side of the larger quarry at Raggedstone, and is unconformably overlain by the Hollybush Sandstone. He has seen nothing like it in any other part of the Malvern Hills. It is something more than a *phyllite*, and reminds one of nothing so much as the Casanna and other younger schists of the Alps. It appears to be a fragment of the lithosphere belonging to a much later Archæan stage of development than the rest of the Malvern Chain, a stage of development in which super-heated water or steam has played a prominent part, rather than to the pyrogenic stage of rock-genesis,[1] to which most of the rocks of the Malvern Chain may be referred. The writer failed in his attempt to trace a gradual transition from them into the gneissose rocks adjoining these schists.

Taking into account the great discordancy that exists between these schists and the overlying Upper Cambrian strata, it appears easier to look upon them as the sole remnant (now exposed to view) of the younger Archæans left by the extensive denudation to which the whole region must have been subjected in early Cambrian and later pre-Cambrian times, rather than as having been "manufactured" simply by those dynamic agencies, through the instrumentality of which the earlier mountain-building movements of the Malvern region were effected, though probably somewhat modified by them.

(c.) *Micaceous phyllolithic rocks.*—An alleged case of conversion of a felsite into a "mica-schist" in the Raggedstone has been described by Dr. Callaway. The writer has observed something of the sort in some of the quartzo-felspathic veins, which are so well-developed in the old quarries at Wind's Point and Little Malvern (near the Roman Catholic Chapel), that they have been instanced over and over again, by writers on the geology of the district, as clear cases of truly-bedded sedimentaries; though a closer examination of them, in the light of experience gained in other parts of the range, makes it far more probable that they are deformed crystallines, rather than worked-up clastic materials. Some of these more felspathic individual veins have acquired a structure (a cleavage-foliation) approaching and simulating true *schistosity*, becoming easily fissile under the hammer, with white mica plainly developed macroscopically on their planes of cleavage. This, no doubt, is a secondary product due to the action of infiltrating water, and formed at the expense of the felspar; but it is difficult to see why such a rock should be designated a *schist* (*stricto sensu*) any more than some laminated sandstones of Mesozoic age, on the lamination-planes of which a similar deposit of secondary mica has taken place. Call them what you will, they are certainly very different things from the older mica-schists of the Alps, in which we meet with well-

[1] Compare "Metamorphism of Rocks," p. 91, where the distinction here implied is clearly drawn.

developed *accessory* minerals, such as kyanite, staurolite, and garnet. It seems doubtful if anything more than "cleavage-foliation" is to be seen in these cases.

(iv.) *Shear-planes.*—These are very conspicuous structural features of the rock-masses exposed to view in the extensive quarries at North Malvern, at West Malvern, and at the Hollybush Pass. They seem to be the "divisional planes" of Mr. Rutley. Occasionally, as in the largest quarry at North Malvern, a rough parallelism may be observed in their arrangement, but more extensive observation shows that this is only a local and accidental circumstance. Generally they cut through the rock-mass in all directions and at all angles to the quarry-face, and the evidence they afford of sliding movements justifies one in regarding them as entirely mechanical in their origin, and due to crushing on a grand scale, acting along planes of weakness in the rock, quite similar to the crush-faulting which has been described by Dr. Stapff in the St. Gothard granite cut through in the railway tunnel.[1] They frequently intersect at all angles what we may call the "master-joints" of the rock, which appear to owe their origin to the shrinkage of the mass as a whole during solidification; and they are not in any way petrographically related to the adjoining holocrystalline, and often massively-crystalline rock. It seems impossible, therefore, to regard them as traces of anything of the nature of bedding. The mineral changes wrought by dynamic action on these divisional planes in the mountain-building stage, appear to be entirely in the direction of degradation of the basic constituents of the adjoining rock into oxides of the heavier metals (chiefly haematite), after pulverization of the rock for the thickness of a few millimetres. They are often conspicuously slickensided. Possibly some of the iron oxide may be present as carbonate, synthesized by the action of carbonated waters from the surface in the presence of organic matter in solution in such waters reducing the peroxides to protoxides; but these results are altogether secondary, and have nothing to do with the genesis of the rocks. The same remark probably applies to epidote, which is rather abundant as an infilling material in the smaller cracks of the diorite (especially at North Malvern), and was derived in all probability by the agency of infiltrating water from the hornblende.

(v.) *Crush-planes and zones.*—Where the original rock was very hornblendic this has been degraded into what some would call a "chloritic-schist," and some of these have been at times described by eminent writers under this term. Yet they can hardly be correctly termed schists at all; for they are so incoherent that it is impossible to get a piece capable of being sliced. They might, perhaps, be described as rotten slate with a very irregular cleavage. The original hornblende appears to have been completely pulverized in the presence of interstitial water, and, by the help of this water (or by water introduced subsequently), to have been wrought into a fine pasty mass, upon which a crude cleavage-structure has been induced by pressure. Several narrow zones of such chloritic masses

[1] See GEOLOGICAL MAGAZINE, 1892, p. 6.

with a phyllolithic structure are cut through in the Wych Pass; and they appear to have been localised by slight faulting. But by far the best example seen is in the quarry just north of the old pumping-station at North Malvern, to which the proprietor directed the writer's attention, as a case of a "blue marl" among the crystalline rocks. An inspection soon revealed the fact that the highly basic rock had been crushed here against a massive multiple vein of quartz, the most powerful development of vein-quartz observed in the whole range. The quartz-vein was itself extensively fractured, and the chlorite, as a fine powder, had worked its way into the fissures; but the two minerals remained completely individualized and distinct, showing no apparent tendency to enter into new chemical relationships under even the intense pressure to which the mass here had been subjected. This observation was repeated several times, after being first made in the early part of one's investigation; and reflection upon the extreme extent to which a hornblendic mass had thus undergone deformation (where, owing to the great power of resistance of the quartz-vein, conditions were exceptionally favourable to local concentration of mechanical force), threw much light upon many facts subsequently observed in other parts of the range, and served as the clue to some of the explanations which have been ventured upon in the present paper.

Repeated observations of the Malvern Crystallines tend to force upon the mind the idea that rocks rich in hornblende, or almost entirely composed of that mineral, yield far more to pressure *without crushing*, than do the more felspathic portions, or even the dolerites. Hence it seems to follow that, where the original rock was very felspathic, either from the presence of more numerous felspathic veins, or from the more abundant distribution of felspar (and in some cases much quartz) through the rock, we get a brecciated mass along a plane of thrust, instead of anything approaching to schistosity. Examples of such are seen in the quarries at North Malvern, and in the cutting on the Wych Road just outside the quarry by Rock Villa. Again, in the same quarry-face (as at Little Malvern and Wind's Point) the more massive diorites have apparently undergone but slight deformation, while the thinner masses, which alternate with the felspathic veins, have been degraded into a rock with a more or less phyllolithic structure, in which chloritic material is abundant as a degradation-product of the original hornblende.

General Conclusions.

1. The field-relations observed among the crystalline rocks of the Malvern Chain point to the conclusion that for the most part their petrological morphology originated in *diagenetic paramorphism* and not in any subsequent metamorphism.

2. As regards the structural features (including schistosity) these have resulted to a large extent from the action of mechanical forces; such results being chiefly of a *metataxic* nature (as the author has defined the meaning of the term elsewhere), exhibiting various phases of deformation of the original crystallines, while the action of

dynamic forces in altering the atomic relationships of the chemical constituents of the rocks, *quâ* the development of new minerals, would appear to have been very limited.

3. In the present state of our knowledge the two most interesting questions presented to us by these rocks would seem to consist (*a*) in determining to what extent such dynamic deformation, as we actually observe, has been prior or posterior to the ultimate solidification of the various parts of the original magma; and (*b*) in determining to what extent simple atomic interactions leading to molecular segregation, supplemented by the action of gravity upon minerals of various densities and by forces of a tidal nature acting upon the magma as a whole, may have caused the vein-structure and the stratiform arrangement, which, whether in its original or in its deformed condition, has constituted one of the most puzzling phenomena presented to the minds of investigators of the Malvern crystallines.

4. The phenomena presented by these rocks as a whole seem to lend no support to the doctrine of "regional metamorphism" as usually understood, since, when field-relations are duly considered, it becomes far easier to explain such simulations of "bedding" as are met with, by mechanical deformation of crystalline rocks, to which a diagenetic stratiform structure had been imparted, than by the hypothesis of reconstruction of crystalline minerals out of clastic materials.

5. The writer, having made free use of the published writings of Phillips, Holl, Rutley, and Callaway, and of the valuable results of microscopic work published by the two gentlemen last named, and having approached the subject in the light of the principles advocated by himself in 1888–9, and since, is happy to find himself to a great extent in accord with Dr. Callaway, though unable to follow him to the full extent in the matter of "dynamic metamorphism."

NOTICES OF MEMOIRS.

Abstracts of Papers Read before the British Association, Edinburgh, August, 1892.

I.—On the "Grampian Series" (Pre-Cambrian Rocks) of the Central Highlands. By Henry Hicks, M.D., F.R.S., Sec. Geol. Soc.

In his address to the Geologists' Association in the year 1883 (Proceedings of the Geologists' Association, vol. viii. p. 270), the author gave the name of "Grampian Series" to a group of rocks which occupy an extensive area in the Central Highlands. He described them briefly as "tender gneisses, bright siliceous schists, chiastolite schists, quartzites and limestones," also some "chloritic schists." He considered them as of pre-Cambrian age, and all the evidence since obtained tends to confirm this view. It is quite possible, of course, that newer rocks may be in places entangled amongst them, and the author pointed out certain lines running from

N.E. to S.W. where there are indications of newer rocks in broken folds, but the majority of those which he claimed as being of pre-Cambrian age are now generally admitted to be older than any of the Palæozoic rocks of that area. The further descriptions of these rocks now given have been prepared with the kind assistance of Professor T. G. Bonney, D.Sc., F.R.S., to whom the author some time since submitted specimens, collected at various points in the year 1880.

From near Ballachulish, etc., a finely-banded, fine-grained micaceous schist, containing apparently a considerable amount of felspathic matter.

From Glen Spean, etc. Fine-grained gneisses, not rich in quartz, but with a considerable amount of black mica, not markedly foliated except as the result of subsequent pressure. All are characterized by a peculiar speckled aspect, the spots being about the size of pins' heads. The felspar varies from a warm to a reddish gray.

From Tyndrum, etc. A somewhat varying series of schists, but with a common facies. Some have but little mica, consisting mainly of quartz and felspar, and pale-gray or reddish in colour; others are very micaceous schists of a lead colour, with sheen surfaces and indications of mineral banding. There are also very quartzose gneisses of a white or pinkish-white colour.

Crianlarich and Killin, etc. Calc mica-schists, with sheen surfaces, due to subsequent pressure, but showing mineral banding. Also fine-grained gneisses like some of those near Tyndrum, but as they have a very marked cleavage foliation they may originally have been somewhat coarser grained. A garnetiferous mica-schist from several places.

Blair Athol, etc. Dark mica-schists, with rather a carbonaceous aspect, and a very marked cleavage foliation. Some show on close examination along the edges a speckled aspect, recalling some of the gneisses mentioned above. There is also a fairly coarsely crystalline limestone, with specks and streaks of green serpentinous material and some scattered pyrite; also a calc mica-schist, modified by pressure, and an important series of quartzose schists similar to those found at Tyndrum, etc.

To the south of this line, as mentioned by the author in another paper (Proceedings of the Geologists' Association, vol. vii. p. 84), "schistose and slaty chloritic rocks become more abundant in association with micaceous rocks," and "everywhere strongly recall to mind the pre-Cambrian rocks of Wales, especially those in Anglesea, and in the Lleyn Promontory." The author insisted that the term "Grampian" was the only suitable name for this group of pre-Cambrian rocks. It was suggested and adopted by him at a time when all these rocks were claimed by the Geological Survey as of Silurian age, and these rocks are nowhere in Britain so well exposed as in the Grampian mountains of Scotland.

II.—Earthquake Phenomena in Japan. Twelfth Report. By Prof. John Milne, F.R.S., F.G.S.

1. *The Gray-Milne Seismograph.*

THE seismograph constructed in 1883—the expense of which was partly defrayed by the British Association—has during the last year given diagrams and records of Earthquakes, from number 1106 to number 1241.

2. *Report on Earthquakes felt in Japan in* 1888 *and* 1889.

In 1888 the number of Earthquakes recorded in Japan was 630, and the total land area shaken was 970,800 square miles. In 1889 the number of Earthquakes was 930, and the area shaken 1,048,200 square miles. On February 5th, 1888, an Earthquake shook 57,600 square miles. Some account is given of Earthquakes which preceded the eruption of Bandaisan, when within 10 minutes 28 square miles of a fertile valley were buried from 30 to 100 feet deep beneath a sea of earth and boulders. Everybody in the district perished. The damming up of the valley has formed a lake 8½ miles long.

On July 28th, 1889, an Earthquake took place in the Southern island. Twenty were killed and seventy-four wounded. All these Earthquakes have been classified with regard to hours, days, months, seasons, etc. They are also grouped according to intensity, direction, etc. It is clear that the vast amount of material which comes in yearly from the 700 stations of observation is capable of being analysed by methods other than those given. The Government Staff is insufficient to carry out more than their usual routine work. To carry it out privately requires access to documents and funds. The nature of new researches which might be made is indicated in the Report. At any moment all this valuable material bearing on Seismology accumulated in Japan, and of which there is no copy, may be lost by fire.

3. *Earth Pulsations.*

So far as the writer is aware, no attempt has been made to determine the character of the movements common to all countries usually called Earth Tremors. By using exceedingly light conical pendulums (made from a needle and a silk fibre)—the pointer being replaced by a small mirror reflecting a ray of light—the writer is inclined to the view that the earth-motions producing movements in this form of apparatus are not elastic vibrations such as might be produced by the beating of a steam hammer, but that they are long wave-like undulations like the swell on an ocean. During the time that these pulsations are continuing it is noticed that they have a definite direction. They are most frequent when the place of observation is crossed by a steep barometric gradient, whether there is wind or whether it is fine. The possible relationship of these movements to the escape of Fire Damp, the swinging of pendulums, etc., is discussed. The records of these phenomena have been photographed, and examples of them accompany the Report.

4. *The Overturning and Fracturing of Columns, Walls, etc.*-

By continuing experiments on the overturning of columns when subjected to earthquake-like motion, we can now state with confidence the acceleration required to overturn any given column by its own inertia. In the experiments on fracturing, all the brick columns and walls snapped at their base. The form of column which when moved back and forth by an Earthquake will offer an equal resistance at all horizontal sections to the effects of its own inertia has been determined.

In ordinary engineering practice the cross-section of piers is practically uniform from the base upwards—short piers near a river bank have the same cross-section as long piers in the centre of the river, etc. A large series of piers for bridges now being built in Japan have been designed in accordance with the rules resulting from our experiments on fracturing.

5. *The Great Earthquake of October 28th*, 1891.

9,960 people were killed, 128,750 dwelling houses were totally destroyed, and in a few seconds Japan lost the equivalent of perhaps 30 million dollars. About twelve million dollars have already been poured into the district for repairs and relief. The movement reached Berlin at the rate of 9,800 feet per second. At Tokyo, 150 miles from the origin, the ground moved in long flat waves, which tilted the water in ponds and caused seismographs to act as angle measurers. These waves had a velocity of about eight feet per second. A few chimneys fell.

The origin was the formation of a fault which can be traced for 40 or 50 miles along the surface. Some 3,000 shocks have been recorded since the first great shock. Mountain slopes have been stripped by land slips, valleys are dammed up and lakes have been formed. Valleys have been compressed so that farms have decreased in area. 400 miles of river banks were shaken down or deeply crevassed. Mud volcanoes formed. Railway lines and bridges twisted and distorted. Foundations of bridges in one case were shifted 19 feet.

Some thousands of calculations respecting accelerations to produce fracturing and overturning have been made. Records of seismographs have been examined and the velocity of gravity and of elastic waves have been computed. The origin was in a non-volcanic district but where elevation was in progress. Earthquakes seldom originate from volcanoes, but they occur in volcanic countries where secular movements are in progress or where mountains are in process of formation.

A photographic record taken at each end of a water level several miles in length and if possible at right angles to an axis of elevation might give measurements of slow tilting and throw light on a possible relationship between Earthquakes and these movements, etc. Such an experiment might cost £500.

Earthquake and Volcanic effects will, if possible, be illustrated by some very large photographs kindly *lent* by Prof. W. K. Burton.

The grant last year was £10, but at least three times this sum has been spent.

Before the end of this year, with the assistance of subscribers, I shall publish a Seismological Journal which will be uniform in character with the Transactions of the Seismological Society.

III.—Fossil Arctic Plants found near Edinburgh. By Clement Reid, F.L.S., F.G.S.

RECENT discoveries by Mr. Bennie, of the Geological Survey, have brought to light a series of silted-up tarns or small lochs in the neighbourhood of Edinburgh. These tarns seem to have lain in irregular hollows left on the retreat of the ice, for the lowest deposits usually yield remains of arctic plants. The principal localities for these plants are Corstorphine and Hailes. Trees, except perhaps the alder, are entirely missing in the lower deposits, and the vegetation consists mainly of dwarf willow and birch, with a few herbaceous plants, of species still living within the Arctic Circle. The list now includes the following plants, those marked with an asterisk being arctic species no longer living in the lowlands of Scotland :—

Ranunculus aquatilis, Linn.
,, *repens*, Linn.
Viola (?).
Stellaria media, Cyr.
Rubus sp.
**Dryas octopetala*, Linn.
Potentilla sp.
Poterium sp.
Hippuris vulgaris, Linn.
Myriophyllum spicatum, Linn.
Taraxacum officinale, Web.
Andromeda Polifolia, Linn.
**Loiseleuria procumbens*, Desv.
Menyanthes trifoliata, Linn.
**Oxyria digyna*, Hill.
**Betula nana*, Linn.
Alnus (?).
Salix repens, Linn.
* ,, *herbacea*, Linn.
* ,, *polaris*, Wahlb.
* ,, *reticulata*, Linn.
Empetrum nigrum, Linn.
Potamogeton sp.
Eleocharis palustris, R. Br.
Scirpus pauciflorus, Lightf.
,, *lacustris*, Linn.
Carex, 2 sp.

IV.—Devon and Cornish Granites. By W. A. E. Ussher, F.G.S.

FROM the relations of the stratified rocks to the granites of Devon and Cornwall there is no obtainable evidence as to the upheaval of the latter.

From evidences of great mechanical disturbance (such as deflections of strike and constrictions of outcrop), of metamorphism in areas bordering the granites, from the shapes, relative positions, and internal structure of the granite masses; from the distribution of the Elvans, and from evidences of the production of cleavage in the area prior to the contact metamorphism of the cleaved rocks, it appears that the sites of the Devon and Cornish granite masses were occupied by the granites or pre-existent and subterraneously connected rocks of pre-Devonian age, which had, in a rigid state, exercised an obstructive influence on the north and south movements, and had thereby produced great mechanical effects on the surrounding strata prior to the alteration of the latter.

The contact alteration of the stratified rocks seems to be coeval

with the metamorphism of these ancient masses and the consequent genesis of the granites in their present form during the later stages, or at the close of the Carboniferous epoch. The intrusion of granitoid rocks perhaps accompanied, certainly succeeded, the solidification of the granites, and continued at intervals down to the Permian quartz porphyries. These rocks, called Elvan dykes, approximate, with some few exceptions (notably the north and south Elvan of Watergate Bay), to the general strike produced by the north and south movements, and in some cases, as near Camelford, to the main strike deflections produced by the resistance of granite masses to these movements, but in proceeding from granite to killas they ignore the slight uptilt of the latter on the margin of the granites.

The evidences in favour of the subterranean connection of the Devon and Cornish granites are too strong to be ignored, and this connection annihilates the application of the laccolitic hypothesis advanced by me to account for the relations of the Dartmoor granite, and at the same time contradicts the suggestion of the upheaval of the granites in or through their surroundings.

V.—On the Occurrence of *Chonetes Pratti*, Davidson, in the Carboniferous Rocks of Western Australia. By R. Bullen Newton, F.G.S., British Museum (Natural History).

In this communication the author directed attention to some valves of a *Chonetes* recently discovered by Mr. Harry Page Woodward, F.G.S., in rocks of Carboniferous age situated in the Irwin River District of Western Australia, which he referred to *C. Pratti*, a species described and figured by the late Dr. Thomas Davidson in the "Geologist" for 1859, Plate IV, Figs. 9–12, p. 116. As the original description, contained only in the explanation of the plates, is necessarily somewhat brief and imperfect, the following additional characters were submitted:—

(1) That the external surface of both valves, besides being ornamented with very fine radiating striæ, possess subimbricating concentric lines of growth; (2) that the extent of the cardinal margin represents the minimum width of the shell; (3) that the granular asperites on the interiors of the valves are disposed in lines as they reach the margins, having more or less an elongate appearance resembling short tubular spines; (4) that the external surface of the ventral valve exhibits a number of small orifices placed at irregular distances, which are probably basal attachments of spines, a character known to exist in *Chonetes papilionacea*, *C. Hardrensis*, etc. The author then gave the dimensions of the Davidson type valves, together with those of the Western Australian specimens, the latter being somewhat larger. A minute examination of the Western Australian specimens and their comparison with the originals of *C. Pratti* in the Davidson collection at the British Museum has demonstrated the fact that they are mineralogically as well as structurally the same brachiopod. This fact is of considerable importance, as the Davidson specimens at the time of descrip-

tion were unlocalised, and the species, as far as can be ascertained, has never been referred to since either by Davidson himself or by any other writer up to the present time. There is very little doubt that the new material from the Irwin River District yielded also the Davidson types. The paper concluded with a small list of Carboniferous fossils collected in the same neighbourhood, which have been described and figured by Messrs. A. H. Foord and G. J. Hinde in the GEOLOGICAL MAGAZINE for 1890.

VI.—THE FULLER'S EARTH MINING CO. AT WOBURN SANDS. By A. C. G. CAMERON, Geological Survey of England and Wales.

SINCE reporting to the British Association in 1884 and 1891 on the progress made in working this mineral, the demand for it has gone on steadily increasing, and mining on systematic principles has been established in Bedfordshire for the first time. The mines now show an extensive industry, with underground galleries that extend many hundreds of feet. The layers of earth as they come to be worked are not found disposed quite evenly, but raised into slight inequalities, ridge-and-furrow-like. Although all one sort of earth, the layers alternate in colour downwards, from yellow, through blue, to yellow again; a difference in colour which Mr. Player, who has analysed the Woburn earth, does not consider is explained by difference in composition.

It has long since been suggested that the name "Woburn Sands" should be applied to the lower portion of the Lower Greensand of the Midlands, and the name may well be retained for Bedfordshire and Bucks. It is at Woburn Sands, in these counties, that the greatest expanse and greatest thickness of Greensand occurs, and where it contains also those valuable deposits of fuller's earth.

VII.—NOTE ON A GREEN SAND IN THE LOWER GREENSAND, AND ON A GREEN SANDSTONE IN BEDFORDSHIRE. By A. C. G. CAMERON, Geological Survey of England and Wales.

THE beds in the section at the "Parish Sandpit" at Apsley Guise are given below in descending order:—

	Ft.	In.
1. Yellow and grey sand with strings of yellow fuller's earth ...	20	0
2. Lenticular seam yellow fuller's earth...	3 to	6
3. Yellow and grey sand, in parts false-bedded	25	0
4. Yellow fuller's earth, dovetailed amongst green sand	2	0
5. Yellow ochre	0	2
6. Bright green sand 'hearted' darker green	2	6
7. Coarse, buff-coloured irony sand	1	8
Oxford Clay	54	10

The bright green sand (No. 6), with a darker middle portion, consists of irregular-shaped grains of quartz, stained green; besides which, there are brown grains, the precise nature of which remains for the present undetermined. With the hammer this sand gives a brown streak, the brown grains, which are comparatively soft, being the cause of it. The absence of glauconite is a distinct feature in

this sand, that being the usual colouring-matter of cretaceous green sands. A darker tint of green pervades the middle portion of the bed, giving it the appearance of being 'hearted,' as the expression goes. Rather over a mile from this place, Mr. Whitaker observed in the tower of Husborne Crawley Church a quantity of green sandstone of a bright colour, and sometimes of a glassy texture, which has been recognized as like some that occurs in the Lower Greensand of Ightham, Kent. It is a serviceable-looking stone, and the brightness of its colour adds beauty to the brown sandstone, of which the edifice is mainly built. The stone seems to be a counterpart of the green sand in the Apsley Section, and similarly colour-hearted. Professor Bonney, on receiving specimens of the Husborne Crawley rock, but speaking from sight only, doubts, however, whether it is the same as the green sand at Apsley. It may be mentioned that pieces of the same rock have been dug up in the roadway half a mile from the church, and a larger boulder-like piece lies by the roadside, on the outskirts of the village green. Adjoining the churchyard is a very old-looking excavation, that suggests the spot at which this stone may have been got. Seeing the difficulty of transporting stones in olden times, it is extremely unlikely that the stone came from a distance. Possibly, therefore, there may be some local equivalent of the Lower Greensand of Kent in the Bedfordshire Greensand, which, if not entirely dug away in supplying the stone for Crawley Church, may yet again be brought to light.

VIII.—British Association for the Advancement of Science, Edinburgh, 1892.

Address to the Geological Section by Professor C. Lapworth, LL.D., F.R.S., F.G.S., President of the Section.

(PART II.)

In the structure of our modern mountain ranges we discover the most beautiful illustrations of the bending and folding of the rocky formations of the earth-crust. The early results of Rogers among the Alleghanies, and of Lory and Favre in the Western Alps, have been greatly extended of late years by the discoveries of Heim and Baltzer in the Central Alps, of Bertrand in Provence, of Margerie in Languedoc, of Dutton and his colleagues in the western ranges of America, and of Peach and Horne and others in the older rocks of Britain. The light these researches throw upon the phenomena of mountain structure will be found admirably summarised and discussed in the works of Leconte, of Dana, of Daubrée, of Reade, of Heim, and finally in the magnificent work of Suess, the 'Antlitz der Erde,' of which only the first two volumes have yet appeared.

Looking first at the mountain fold in its simplest form as that of a bent rock-plate composed of many layers, which has been forced into two similar arc-like forms, the convexities of which are turned, the one upwards and the other downwards, we find in the present mountain ranges of the globe every kind represented. We commence with one in which the arch is represented merely by a gentle swell in the rock sheet, and the trough by an answering shallow depression, the two shading into each other in an area of contrary flexure. From this type we pass insensibly to others in which we see that the sides of the common limb or septum are practically perpendicular. From these we pass to folds in which the twisted common limb or septum overhangs the vertical, and so on to that final extreme, where the arch limb has been pushed completely over on to the trough limb, and all three members, as in our note-book experiment, are practically welded into one conformable solid mass.

Although the movements of these mountain folds are slow and insensible, and only effected in the course of ages, so that little or no evidence of the actual movement of any single one of them has been detected since they were first studied, yet it is perfectly plain that when we regard them collectively, we have here crust folds in every stage of their existence. Each example in itself represents some one single stage in the lifetime of a single fold. They are simply crust folds of different ages. Some are, as it were, just born ; others are in their earliest youth. Some have attained their majority, some are in the prime of life, and some are in the decrepit stages of old age. Finally, those in which all three members—arch limb, trough limb, and septum—are crushed together into a conformable mass, are dead. Their life of individual movement is over. If the earth pressure increases, the material which they have packed together may of course form a passive part of a later fold, but they themselves can move no more.

In many cases, due partly to the action of longitudinal pressures, the septum becomes reduced to a *plane* of contrary motion, namely—the over-fault, or thrust-plane, and the arch limb and the trough limb slide past each other as two solid masses. But here we have no longer a fold, but a fault.

We see that every mountain fold commences first as a gentle alternate elevation and depression of one or more of the component sheets of the geological formations which make up the earth-crust. This movement is due apparently to the tangential thrusts set up by the creeping together, as it were, of those neighbouring and more resistant parts of the earth-crust which lie in front of and behind the moving wave. Yielding slowly to these lateral thrusts, the crest of the fold rises higher and higher, the trough sinks lower and lower, the central common limb or septum grows more and more vertical, and becomes more and more strained, sheared, and twisted. As this middle limb yields, the rising arch part of the fold is forced gradually over on to the sinking trough part, until at last all three members come into conformable contact, and further folding as such is impossible. Movement ceases, the fold is dead. We see also from our note-book experiment that the final result of the completion of the fold is clearly to strengthen up and consolidate that part of the crust plate to the local weakness of which it actually owed its origin and position. The fold has, by its life-action, theoretically trebled the thickness of that part of the earth-plate in which its dead remains now lie. If the lateral pressure goes on increasing and the layers of the earth-crust again begin to fold in the same region, the inert remains of the first fold can only move as a passive part of a newer fold : either as a part of the new arch-limb, the new trough-limb, or the new septum. As each younger and younger fold formed in this way necessarily includes a more resistant, and therefore a thicker, broader, and deeper sheet of the earth-crust, we have here the phylogenetic evolution of a whole family of crust folds, each successive member of which is of a higher grade than its immediate predecessor.

But it very rarely happens that the continuous crust plate in which any fold is imbedded is able to resist the crust creep until the death of the first fold. Usually, long before the first simple fold is completed, a new and a parallel one rises in front of it, normally on the side of the trough limb, and the two grow, as it were, henceforward side by side. But the younger fold, being due to a greater pressure than the older, must of necessity be of a higher specific grade, and the two together form a generic fold in common.

Our present mountain systems are all constituted of several families of folds, formed in this way of different gradations of size, of different dates of origin, and of different stages of life evolution ; and in each family group the members are related to each other by this natural genetic affinity.

Sometimes the new folds are formed in successive order on one side of the first fold, and then we have our unilateral (or so-called unsymmetrical) mountain groups, like those of the Jura and the Bavarian Alps. Sometimes they are formed on both sides of the original fold, and then we have our bilateral (or so-called symmetrical) ranges, like the Central Alps. In both cases the septa of the aged or dead folds of necessity all slope inwards towards the primary fold. If, therefore, they originate only on one side of the primary fold, our mountain group looks unsymmetrical, with a very steep side opposed to a gently sloping side. If they grow on both sides of the original fold, we have the well-known 'fan structure' of mountain ranges. In this latter case the whole complex range is seen at a glance

to be a vast compound arch of the upper layers of the earth-crust, keyed up by the material of the dead or dying folds, which by the necessities of the case constitute mighty wedges whose apices are directed inwards towards the centres of the system. But a complete arch of this kind is in reality not a single compound fold, but a double one, with a septum on both sides of it, and it requires two troughs, one on each side of it, as its natural double complement. The so-called unsymmetrical ranges, therefore, which are theoretically constituted merely of arch limb, trough limb, and septum, are locally the more natural and the more common.

It is clear that in the lifetime of any single fold its period of greatest energy and most rapid movement must be that of middle life. In early youth and in old age the lateral pressure is applied at a very small angle, and the tangential forces act therefore under the most disadvantageous circumstances. But in the middle life of the fold the arch limb and the trough limb stand at right angles to the septum, and the work of deformation is then accomplished under the most favourable mechanical conditions and with the greatest rapidity. That is to say, the activity of the fold and the rate of movement of the septum, like the speed of the storm wind, varies directly as the gradient.

In our note-book experiment we observed that little or no change took place in the arch limb and trough limb, while the septum became remarkably sheared and twisted. The same is the case in nature, but here we have to recollect that these moving mountain folds are of enormous size, indeed actual mountains in themselves. These great arches, scores of miles in length, thousands of feet in height and thickness, must of necessity be of enormous weight, capable of crushing to powder the hardest rocks over which they move, while the thrust which drives them forward is practically irresistible. It is plain, therefore, that while the great arch limb and the trough limb of one of these mighty folds move over and under each other from opposite directions, they form in combination an enormous machine, composed of two mighty rollers or millstones, which mangle, roll, tear, squeeze, and twist the rocky material of the middle limb or septum, which lies jammed in between them, into a laminated mass. This deformed material, which is the characteristic product of the mountain-making forces, is, of course, made up of the stuff or the original middle limb of the fold; and whether we call it breccia, mylonite, phyllite, or schist, although it may be composed of sedimentary stuff, it is certainly no longer a *stratified* rock; and though it may have been originally purely igneous material, it is certainly no longer *volcanic*. It is now a manufactured article made in the great earth mill.

These mountain folds, however, are merely the types of folds and wrinkles of all dimensions which affect the rock formations of the earth crust. Within the mountain chains themselves we can follow them in lesser and lesser dimensions, fold within fold, first down to formations, then to strata, then to laminæ, till they disappear at last in microscopic minuteness beyond the limits of ordinary vision. Leaving these, however, for the moment, let us travel rather in the opposite direction, for these mountain folds are by no means the largest known to the stratigraphical geologist. Look at any geological section crossing our type continent of North America, and it will be found that the whole of the Rocky Mountain range on its western side and the Alleghany range on the east are really two mighty compound geological anticlines, while the broad sag of the intermediate Mississippi Basin is actually a compound geological syncline made up of the whole pile of the geological formations. That is to say, the continent of North America is composed of a pair of geological folds, the two arches of which are represented by the Rockies on the one side and the Alleghanies on the other, while the intermediate Mississippi syncline is the common property of both. Here, then, we reach a much higher grade of fold than the orographic or mountain-making fold, viz., the plateau-making fold or the semi-continental fold, which, because of its enormous breadth, must include a very much thicker portion of the earth-crust than the ordinary orographic fold itself.

But which must be the real middle limbs of these two American folds, the septal areas where most work is now being done and the motion is greatest?

Taught by what we have already learned of the mountain wave, the answer is immediate and certain. They must be on the steeper sides of each of the two folds, namely on those which face the ocean. How perfectly this agrees with the geological facts goes without saying. It is on the steep Pacific side of the western fold that

the crushing and crumpling of its rocks is the greatest. It is on the Atlantic side of the eastern fold that the contortion and metamorphism of its rocks are at their maximum, while in the common and gently sloping trough of both folds, namely, in the intermediate Mississippi Valley, the entire geological sequence remains practically unmodified throughout.

Again, which of these two American folds should be the more active at the present day? Taught by our study of the mountain wave, the answer again is immediate and conclusive. It must be that fold whose septum has the steeper gradient. Geology and geography flash into combination. The steeper Pacific septum of the western fold from Cape Horn almost to Alaska is ablaze with volcanoes or creeping with earthquakes, while the gently inclined Atlantic septum of the eastern fold from Greenland to Magellan Straits shows none, except on the outer edge of the Antilles, in the very region where the slope of the surface is the steepest. We see at a glance that the vigour of these two great continental folds, like those of our mountain waves, varies directly as the surface gradient of the septum.

But the geographical surface of North America, considered as a whole, is in reality that of a double arch with a sag or common trough in the middle. We have seen already that this double arch, must be regarded as the natural complement of the equally double Atlantic trough. Here, then, if the path of analogy we have hitherto so triumphantly followed up to this point is still to guide us, the trough of the Atlantic must be, not only in appearance but in actuality, formed of two long minor folds of the same grade as the two that form the framework of America, but with their members arranged in reverse order. If so, their submarine septa ought also to be lines of movement and of volcanic action. And this is again the case. The volcanic islands of the Azores and St. Helena lie not exactly on the longitudinal crests of the mid-oceanic *Challenger* ridge, but upon its bounding flanks.

But we have not yet, however, finished with our simple folds. If we draw a line completely round the globe, crossing the Atlantic trough at its shallowest, between Cape Verde and Cape St. Roque, and continued in the direction of Japan, where the Pacific is at its deepest, we find that we have before us a crust fold of the very grandest order. We have one mighty continental arch stretching from Japan to Chili, broken submedially by the sag of the Atlantic trough; and we see that this great terrestrial arch stands directly opposed to its natural complement, the great trough of the Pacific, which is bent up in the middle by the mightiest of all the submarine buckles of the earth-crust, on which stand the oceanic islands of the central Pacific.

But if this be true, then the septum of all septa on our present earth-crust must cross our grandest earth fold where the very steepest gradient occurs along this line, and it must constitute the centre-point of the moving earth fold, and of greatest present volcanic activity. And where is this most sudden of all depression? Taught once more by our geological fold, the answer is instantaneous and incontrovertible. It is on the shores of Japan, the mightiest and most active of all the living and moving volcanic localities on the face of our globe.

But the course of the line which we indicated as forming our grandest terrestrial folds returns upon itself. It is an endless fold, an endless band, the common possession of two sciences. It is geological in origin, geographical in effect. It is the *wedding-ring* of geology and geography, uniting them at once and for ever in indissoluble union.

Such an endless fold again must have an endless septum, which, in the nature of things, must cross it twice. Need I point out to the merest tyro in these wedded sciences that if we unite the Old and New Worlds and Australia, with their intermediate sags of the Atlantic and Indian Oceans, as one imperial earth arch, and regard the unbroken watery expanse of the Pacific as its complementary depression, then the circular coastal band of contrary surface flexure between them should constitute the moving master septum of the earth's crust. This is the "Volcanic Girdle of the Pacific," our "Terrestrial Ring of Fire."

Or, finally, if we rather regard the compact arch of the Old World itself as the natural complement of the broken Indo-Pacific depression, then the most active and continuous septal band of the present day should divide them. Again our law asserts itself triumphantly. It is the great volcanic and earthquake band

on which are strung the Festoon Islands of Western Asia;—the band of Mount St. Elias, the Aleutians, Kamtchatka, and the Kuriles;—the band of Fusijama, Krakatoa, and Sangir. The rate of movement of the earth's surface doubtless everywhere varies directly as the gradient.

We find, therefore, that even if we restrict our observations to the most simple and elementary conception of the rock fold as being made up of arch-limb, trough-limb, and twisting but still continuous septum, we are able to connect, in one unbroken chain, the minutest wrinkle of the finest lamina of a geological formation with the grandest geographical phenomena on the face of our globe.

We find, precisely as we anticipated, that the wave-like surface of the earth of the present day reflects in its entirety the wave-like arrangement of the geological formations below. On the land we find that the surface arches and troughs answer precisely to the grander regional anticlines and synclines of the subterranean sedimentary sequence; and it may, I believe, be regarded as certain that the submarine undulations have a similar or complementary relationship. We find in the New Geology, as Hutton found in the Old, that geography and geology are one. We find, as we suspected, that the physiognomy of the face of our globe is an unerring index of the solid personality beneath. It bears in its lineaments the characteristic family features and the common traits of its long line of geological ancestors.

Such, it seems to me, is an imperfect account of the introductory paragraphs of that great chapter in the New Geology now in course of interpretation by geologists of the present day; and we have translated them exactly in the old way by the aid of the only living geological language, namely, the language of present natural phenomena, and I doubt not that sooner or later the rest of this great chapter will be read by the same simple means.

I have strictly confined myself to-day to the discussion of the characteristics of the simple geological fold as reduced to its most elementary terms of arch, trough, and unbroken septum; for this being clearly understood, the rest naturally follows. But this twisted plate is really the key which opens the entire treasure-house of the *New Geology*, in which lie spread around in bewildering confusion facts, problems, and conclusions enough to keep the young geologist and other scientific men busily at work for many a long year to come.

Into this treasure-house I often wander myself, in the few leisure hours that I can steal from a very busy professional life; and out of it I bring now and again heresies that sometimes amuse and sometimes horrify my geological friends. As you have so patiently listened to what I have already said, perhaps you will permit me in a few final sentences to indicate in brief some of those novelties which I see already more or less clearly, and a few of those less novel points on which it appears to me that more light is wanted. My excuse is twofold, first, to furnish material for work and controversy to the young geologists; and second, to obtain aid for myself from workers in other walks of science.

The account of the simple rock-fold which I have already given you is of the most elementary kind. It presupposes merely the yielding to tangential pressure from front and back, combined with effectual resistance to sliding. But in the layers of the earth-crust there is always in addition a set of tangential pressures theoretically at right angles to this. The simple fold becomes a *folded fold*, and the compound septum twists not only vertically but laterally. On the surface of the globe the double set of longitudinal and transverse waves brought about in this way is everywhere apparent. They account for the detailed disposition of our lands and our waters, for our present coastal forms, for the direction, length, and disposition of our mountain-ranges, our seas, our plains, and lakes. The compound arch becomes a dome, its complementary trough becomes a basin. The elevations and depressions, major and minor, are usually twinned, like the twins of the mineralogist, the complementary parts being often inverted, and turned through 180° (compare Italy with the Po-Adriatic depression). Every upward swirl and eddy has its answering downward swirl. The whole surface of our globe is thus broken up into fairly continuous and paired masses, divided from each other by moving areas and lines of mountain making and crust movement, so that the surface of the earth of the present day seems to stand midway in its structure and appearance between the surfaces of the sun and the moon, its eddies wanting the mobility of

those of the one and the symmetry of those of the other. In the geology of the earth-crust, also, the inter-crossing of the two sets of folds, theoretically at right angles to each other, gives rise to effects equally startling. It lies at the origin of the thrust-plane or overfault, where the septal region of contrary motion in the fold becomes reduced to, or is represented by, a *plane* of contrary motion. It allows us to connect together under one set of homologies folds and faults. The downthrow side of the fault answers to the trough, the upthrow side to the arch, of our longitudinal fold, while the fault-plane itself represents the septal area reduced to zero. The node of the fault, and the alternation and alteration of throw, are due to the effects of the transverse folding.

These transverse folds of different grades, which affect the various layers of the earth-crust differentially, account also for the formation of laccolites, of granitic cores, and of petrological provinces. They enable us also to understand many of the phenomena of metamorphism.

Of the folds of the *third order*, I shall here say nothing, but I must frankly admit that the primal cause of all this tangential movement and folding stress is still as mysterious to me as ever. I incline to think that the motion is due to many causes—to tidal action, to sedimentation, and many others. I cannot deny, however, that it may be *mainly* the result of the contraction in diameter of our earth, due to the loss of its original heat into outer space? For everywhere we find evidences of symmetrical crushing of the earth-crust by tangential stresses. Everywhere we find proofs that the various layers of that crust have been most affected differentially, and the outer layers have been bent the most. We seem to be dealing not so much with a solid globe as with a globular shell composed of many layers.

Is it not just possible after all that, as others have suggested, our earth is such a hollow shell, or series of concentric shells, on the surface of which gravity is at a maximum, and in whose deepest interior it is non-existent? May this not be so, also, in the case of the sun, through whose spot-eddies we possibly look into a hollow interior? If so, perhaps our present nebulæ may also be hollow shells formed of meteorites; on the surfaces of these shells the fiery spirals we see would be the swirls which answer to the many twisting crustal septa of the earth. Our comets, too, in this case might be elongated ellipsoids, whose visible parts would be merely interference phenomena, or sheets of differential movement.

In this case we have represented before us to-day the past of our earth as well as its present. Uniformity and Evolution are one.

Thus from the microscopic septa of the laminæ of the geological formations we pass outwards *in fact* to these moving septa of our globe, marked on the land by our new mountain chains, and on the shores by our active volcanoes. Thence we sweep, *in imagination*, to the fiery eddies of the sun, and thence to the glowing swirls of the nebulæ; and so outwards and upwards to that most glorious septum of all the visible creation, the radiant ring of the Milky Way.

Professor George Darwin, in his Address to the section of Mathematical and Physical Science at the meeting of the British Association at Birmingham in 1886, with all the courage of genius, and the authority of one of the sons of the prophets, acknowledged that it seems as likely that "meteorology and geology will pass the word of command to cosmical physics as the converse." Behind this generous admission I shelter myself. But I feel absolutely confident that long after the physicists may have swept away these astronomical suggestions as "the baseless fabric of a vision," there will still remain in the treasure-house of the geological fold a wealth of abundant material for the use of the mathematician, the physicist, the chemist, the mineralogist, and the astronomer, of the deepest interest and of the highest value.

NOTE.—The first Part of the above Address was set up inadvertently from an uncorrected proof. Most of its errors explain themselves, but for the sake of clearness, the reader should substitute the following for the corresponding paragraphs on pp. 420–421.

In the earlier days of geology one of the first points recognised by our stratigraphists was the fact that the formations were successive lithological sheets, whose truncated outcropping edges formed the present surface of the land, and that these sheets lay inclined at an angle one over the other, as William Smith quaintly

expressed it, like a tilted 'pile of slices of bread and butter.' But as discovery progressed the explanation of this arrangement soon became evident. The formations revealed themselves as a series of what had originally been deposited as horizontal sheets, lying in regular order one over the other, but which had been subsequently bent up into alternating arches and troughs (*i.e.* the anticlines and synclines of the geologist), their visible parts, which now constitute the surface of our habitable lands, are simply those parts of the formations which are cut by the irregular plane of the present earth's surface. All those parts of the great arches and troughs formerly occurring above that plane have been removed by denudation; all those parts below that plane lie buried still out of sight within the solid earth-crust.

Although in every geological section of sufficient extent it was seen that the anticline or arch never occurred without the syncline or trough—in other words, that there was never a rise without a corresponding fall of the stratum. Yet it is only of late years that the stratigraphical geologist has come clearly to recognize the fact that the anticline and syncline must be considered together, and must be united as a single crust-wave, for the arch is never present without its complementary trough, and the two together constitute the *tectonic, structural, or orographic unit*, namely, *The Fold*, the study of which, so brilliantly inaugurated by Heim in his "Mechanismus der Gebirgsbildung," is destined, I believe, in time, to give us the clue to the laws which rule in the local elevation and depression of the earth-crust, and furnish us with the means of discovery of the occult causes that lie at the source of those superficial irregularities which give to the face of our globe its variety, its beauty, and its habitability.—Edit. Geol. Mag.

REVIEWS.

Edinburgh Museum of Science and Art Guide to the Geological Gallery. 8vo. Niel & Co., Edinburgh, and H. M. Stationery Office, 1892.

THE Science and Art Department having sanctioned the removal of the Collection made by the Geological Survey of Scotland, and its rearrangement in the Grand Museum of Science and Art in Edinburgh, it can now be most advantageously studied. An extensive gallery, the upper floor of the west wing, is devoted to the exhibition of minerals, rocks, and fossils, not only those collected by the Survey, but those given by Messrs. Dudgeon and Milne, Prof. Heddle, Dr. Wilson, and others, and illustrative of the structure and geological history of Scotland. The rock-specimens are arranged, together with the Geological Maps, as far as possible according to their respective Counties; and the rocks are most fully displayed under the district where they are most typically represented, with ample references, by means of numbers and otherwise, to the localities. Descriptive labels for the petrology, geology, and topography accompany the specimens. An index-collection of rock-types is also at hand for the use of students.

In the Guide here noticed, the order and contents of the cases illustrative of the petrological geology of the several Counties of Scotland are given in detail. The stratigraphical collection of Scottish Fossils is similarly treated, but not so fully: and the minerals are briefly noticed. Some geological models and photographs are also mentioned.

The Department of Science and Art has to be congratulated on the

excellent manner in which this new arrangement of the Geological Museum has been carried out, and on the praiseworthy attempt to give every possible opportunity and convenience for the study of Geology in the great Northern Capital.

CORRESPONDENCE.

THE MAMMOTH AND THE GLACIAL DRIFT.

Sir,—From the lofty heights of literary criticism Sir Henry Howorth looks down upon the struggling company of practical geologists, and seems to think that he gains a much better view of the problems to be solved than those who are toiling among the inequalities of the plain below.

The toilers on the plain, however, will be apt to think that they can perceive the structure of these inequalities better than the man who surveys them from such a distant standpoint, and when this person boldly proclaims from his mountain-top that the geologists are making great mistakes they will naturally ask him if he has ever taken a nearer view of the deposits he points to. Now it does not appear that Sir Henry Howorth has had any practical experience as a geologist; he evidently has a considerable acquaintance with the literature of Pleistocene geology, but geologists cannot accept this as a sufficient qualification for dealing with such a difficult subject as the relative ages of British Pleistocene deposits. His lack of practical acquaintance with the deposits he is writing about shows itself on page 400, where he quotes Prof. Flower's discovery of flint implements "at Thetford on the Ouse" as bearing on the age of the gravels in the valley of the Ouse near Bedford! Is it possible that from his lofty standpoint Norfolk and Bedfordshire seem close together?

It is of course perfectly logical to form a theory and then to see if it harmonises with the facts, but if he imagines that he has exhausted the data on which geologists ground their belief that some of the mammaliferous gravels are of later date than the East Anglian Boulder-clays, he is very much mistaken, and his claim to have proved that deposits containing the Mammoth fauna are *never underlain* by Glacial Drift is simply preposterous.

To disprove a universal negative a single case is of course sufficient, and he actually quotes such a case without recognizing it as such. This is the section near Burgh, in Lincolnshire, where gravel with mammalian bones is intercalated between two sheets of Boulder-clay, the lower bed or "marl" being really the main mass of Boulder-clay. Whether my description of the locality fails to make this clear to the reader I cannot say, for I have not a copy of the memoir with me in the country.

I believe, too, though here I do not speak from personal knowledge, that there is no doubt about the superposition of the brick-earth at Hoxne. Mr. H. B. Woodward distinctly states that the section he saw in 1878 had chalky Boulder-clay beneath it,[1] though

[1] Geology of England and Wales, 1887, p. 515.

he admits that small pockets of such clay were also seen above it. Why does Sir Henry Howorth only quote the latter statement and not the former?

If Sir Henry will study the facts in the field, and especially if he will have a few excavations made at any of the localities where the relative age of the beds is doubtful, he will earn the gratitude of geologists, but his present methods of controversy do not entitle him to their respect.

There is an excellent field for research at Brandon; it is easy to prove that some of the brick-earths pass under the Boulder-clay, but there still remain two points to be decided, (1) do such brick-earths contain flint implements? (2) are there not other deposits containing flint implements and mammalian remains which rest on this Boulder-clay?

Let Sir Henry Howorth do for Geology what General Pitt-Rivers has done for Archæology, and we will welcome the results. Meantime any further endeavour to support a preconceived theory by a partial examination of written statements will hardly be welcome to readers of this Magazine.

September 5th, 1892. A. J. JUKES-BROWNE.

SHAPES OF SAND GRAINS.

SIR,—It is pleasant to hear from so experienced an observer as Mr. Cecil Carus-Wilson that the views expressed in my paper on Glacial Geology on the generally superior roundness of Marine Sands as compared with river sands are borne out by his own independent observations.

My remarks on the rounding of sand grains were strictly limited to its bearing on glacial geology. The sand-dunes referred to were those of our own coast. Here from Crosby to Southport we have 23 square miles of Blown sand which I have been living on and working in as an engineer for the last 25 years. I can find no detectable difference in form between the sand grains of the shore and those of the dunes.

Desert sands are of course out of the question in glacial geology, and I quite agree with Mr. Carus-Wilson's observations relative to them. His other interesting observations shall have my attention in future work.

I have found my sand investigations of the greatest use in glacial geology, though not originally undertaken for that purpose. The polish in some of the glacio-marine sand grains is quite remarkable. No glacial *shelly* sands that I have examined fail to show much rounding of the grains—not only those quartz but the undoubted glacially derived materials also. There are also other glacial *shelless* sands of which there are the most convincing evidences of marine origin that exhibit equal evidences of extreme attrition.

The non-marine but purely glacial sands are invariably angular. I have just received from Professor J. J. Stevenson, of New York, a sample of sand from Glacier Bay in front of the Muir Glacier, Alaska, which is remarkably angular in grain.

Like all instruments of research, this one of shape must be used with common sense and the surrounding circumstances taken into consideration, but when as on Moel Tryfaen extremely rounded and polished grains of quartz are found amongst a great mass of very angular material they may be treated as erratics. No rock in the neighbourhood could yield them, and to the educated eye they at once proclaim their sea-origin, whatever mode of transit may be theoretically provided for them according to the proclivities of the geologist.

I am glad of the opportunity of reiterating these views first brought forward in a paper recently read before the Geological Society.

T. Mellard Reade.

Park Corner, Blundellsands, *Sept. 7th*, 1892.

THE ROCKS OF SOUTH DEVON.

Sir,—Now that Mr. A. R. Hunt's three-months-long dissertation on the Devonian Rocks of South Devon has come to an end, I may ask space for a very few words, as I do not intend to discuss the subject in detail.

He attaches importance to mineral coincidences between the schists and the admitted Devonian rocks. Some of these, such as the iron-ores, seem to me very much of a Monmouth-Macedon type; others to be more naturally explained by supposing that the latter have derived some of their materials from the former or a kindred crystalline group, an alternative which seems to me inadequately discussed in his paper.

As I have always held that the dark mica-schists were once sediments, as the Devonian phyllites have been, and I have never denied the possibility that some of the green chlorite-schists originally might have been basic igneous rocks, parts of Mr. Hunt's arguments do not affect my position.

From Mr. Hunt's paper I infer that he is not aware that a schist, after crushing (particularly if dark in colour), is sometimes very difficult to distinguish from a much-squeezed dark slate; also that some other crushed crystalline rocks simulate squeezed grits. The difficulties are local, and generally can be overcome when you know what to look for, but they are so real that I always hesitate to express an opinion on microscopic slides when I have not seen the rock in the field, and even then, once or twice, when the outcrops were scanty, have been unable to come to a conclusion.

I have never denied that what it is now the fashion to call dynamometamorphism has greatly modified both the schists and the Devonian rocks, but, in calling attention to it, I pointed out that the one set "went into the mill" as schists, the other as clays. I do not find that Mr. Hunt has adequately discussed this very important matter.

During the nine years which have elapsed since my paper was written, I have many times examined both my own and other specimens from South Devon, and have had unusual opportunities of studying, in other regions, similar rocks and some sections which

were very helpful in illustrating those of the Start district. So, with a greatly enlarged experience, both in the field and with the microscope, I could now improve my former paper (*e.g.* I could amend the accounts of the "chloritic" rocks; should be more ready to recognise altered basic igneous rocks among them; should say that the mineral, very doubtfully identified with kyanite, and some of the smaller grains of water-clear mineral—thought then to be quartz—were more probably secondary felspars), but I should express myself, if possible, yet more confidently as to the distinction in lithological characters and geological age of the two groups of rocks, the schists and the slaty Devonian system.

Mr. Hunt, so far as I can judge from internal evidence, has had little experience in dealing with problems such as that which he attempts—perhaps the most difficult presented to petrologists. Possibly his experience may be commensurate with my own, but till I have reason to believe that he has studied such problems in other fields than South Devon, and has ample materials at his command for the necessary research, I must decline to do more than say that my original opinion is not in any way altered by his dissertation.

T. G. Bonney.

"CONE-IN-CONE STRUCTURE."

Sir,—In the September Number of the Geological Magazine there is a note by W. S. Gresley, on "Cone-in-cone Structure," in which he refers to "Mr. John Young's theory of how the rock was formed." With your kind permission, I beg to state, that I have *no* "*theory*" on the above subject, and in connection with the explanations that I have given of the cone structure in my paper,[1] the word "theory" is never used in any of my own explanations, but it will be found on p. 25, where I give the opinion of Professor Newberry, who there uses the word "theory" in connection with cone formation, "and the upward escape of gases through a pasty medium." Regarding its formation, all the explanations that I have ventured to give are founded upon what is revealed in the best preserved, and most illustrative specimens of the cone structure that I have found in the carboniferous strata of the West of Scotland, and, I do not think, that in these explanations of the various points of structure, that I have stated anything beyond what the specimens themselves most clearly reveal. I have, in various parts of my paper, pointed out that there are structures which have been referred to "cone-in-cone," but which present appearances so dissimilar to those noticed in my paper, that to them my explanations do not apply, stating, that they will each "have to be described with reference to their external characters and internal structures."

John Young, F.G.S.

Hunterian Museum, University, Glasgow.

[1] Trans. Geol. Soc. Glasgow, vol. viii.